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ON THE EMPLOYMENT OF MATHEMATICAL CURVES AS THE INTRADOS OF ARCHES.

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Rankine, in his work on "Civil Engineering," treats upon the employment of the Hydrostatic arch modified or transformed to suit earth pressure for the profile of a tunnel. This transformed curve he terms the Geostatic arch, and he gives approximate formulæ for its calculation. He also goes pretty fully into the use of the Catenarian curve as a figure for bridges when suitably transformed to suit the depth of load. He does not, however, place the matter in a very clear light, and, as a consequence, it is to be feared that little or no use is made of his deductions in practice, but that too frequently the arches of bridges and tunnels are struck merely to the circular curve without reference to the question of equilibration, the possibly excessive depth of arch ring being sufficient to contain the curve of equilibrium within or near to the middle third of its thickness.

In Rankine's work, also, this subject is treated somewhat disconnectedly, being spread over different pages in various sections of the book, so that considerable labor is involved in collecting the full amount of material necessary to complete the calculation of an arch.

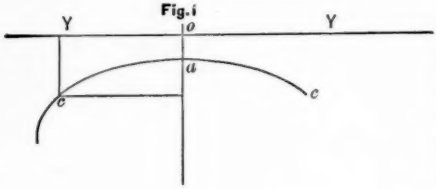
The writer therefore makes no apology for the present essay, for the materials of which he has drawn upon Rankine's "Civil Engineering," but hopes that the examples he has worked out may serve to show the real simplicity and elegance of the methods employed by Rankine, and be of assistance to students who would closely follow the natural line in the design of engineering structures.

The symbols employed are the same as those used in Rankine, which may be of assistance in further study.

Commencing with the tunnel, the intrados of which should be a transformed hydrostatic arch, the words used by Rankine may be quoted here. Describing the hydrostatic arch he says it "is a linear arch or rib, suited for sustaining normal pressure at each point proportional, like that of a liquid in repose to the depth below a given horizontal plane.

"The radius of curvature at the given point in the hydrostatic arch being inversely proportional to the intensity of the pressure, is also inversely proportional to the depth below the horizontal plane at which vertical ordinates representing that intensity commence." Thus, if *c.c* be a submerged arch, and YY be the surface of the water above it, the radius at the point *a* will bear the ratio to the radius at $c = \frac{Yc}{oa}$, being greater than the radius at *c* in proportion as it is nearer to the surface. This arch then is

Fig. 1



exposed to a constant thrust throughout every part, the intensity of the thrust being simply the product of the intensity of pressure at any point by the radius of curvature at that point, and the arch can yield only by the crushing of the material of which it is composed.

Liquid presses equally in every direction, but the pressure of earth varies according to its nature, and the intensity of the lateral pressure of earth upon a buried arch is less than the vertical pressure in a ratio dependent upon the angle of repose φ of the material comprising the strata through which the tunnel passes.

Calling px the vertical pressure and py the horizontal pressure against a tunnel or culvert, the width of a hydrostatic arch suitable for a liquid load, the intensity of which is px at the crown, must be contracted in the ratio $c=\sqrt{\frac{py}{px}}$ at points below the crown when intended to withstand earth pressures. The transformed archway is therefore much less in width than its corresponding hydrostatic linear arch, the reduction being greater in the more stable earths than in those whose angle of repose is low, *e*, near the horizontal. Calling the angle of repose of earth φ the value c may be thus written:

$$C=\sqrt[2]{\frac{Py}{px}}=\sqrt[2]{\frac{(1-\text{Sin}:\varphi)}{(1+\text{Sin}\ \varphi)}}$$

Now, Rankine states that in designing a tunnel by transformation from the hydrostatic arch, knowing the depth x_0 of the tunnel crown below the origin of pressure, u the surface above, and also the depth x_1 of the greatest horizontal diameter below the same datum, it is merely requisite to design the hydrostatic arch as for fluid pressure, and then to contract the horizontal ordinates in the ratio $\frac{c}{1}$ to obtain the profile of the tunnel.

This would certainly obtain such a tunnel as would be perfectly safe, and it might happen that the various dimensions would be correct also; but this would not always be the case, in fact seldom so.

As a rule—an almost invariable rule—the data given to the designer would be the width and the height of the tunnel, and the depth x_0 of the crown below the surface would also be known.

If then, the greatest horizontal diameter of the tunnel was taken at the floor, this would give with a height from floor to crown $=h$, $x_1=x_0+h$.

Now, the width of the tunnel is fixed by two axiomatic rules. In the first place it must be sufficient to allow of the required traffic through the tunnel, whether this be by boat, or one or more railway trains, or otherwise. In the second place, it is requisite, as a matter of structural economy, both in excavating and masonry, that the width should not exceed the minimum safe dimensions.

But the ratio $\frac{c}{1}$ of the $\frac{\text{Geostatic ordinate}}{\text{Hydrostatic ordinate}}$ is a fixed quantity depending upon the frictional stability of the earth through which the tunnel is pierced, and if x_0 and x_1 are also fixed there would be no escape from the necessity of making the tunnel either too wide or too narrow, unless the alternative of increasing or diminishing the height were resorted to.

If such a way out of the difficulty had to be followed, the only two methods practically available would be an increase in either height or width to maintain a minimum possible traffic requirement of with, width a corresponding increase in cost of excavating and masonry or brickwork.

To the student of Rankine, the omission on his part to indicate the method of procedure is often embarrassing, but the solution is simple, and consists in fixing the greatest horizontal diameter above the floor line instead of at that level. The total height of the tunnel being h, if we made $x_1=x_0+h$ we should find that our geostatic arch, unless in very stiff material, would be far wider than we required, but by making $x_1=x_0+a$ we may obtain any suitable width we please, a being anything less than h, and representing the distance of the greatest

horizontal width of tunnel below the crown of the archway.

It is a difficult problem to accurately calculate the hydrostatic arch, but we may make our calculations with a degree of exactness quite sufficient for practical purposes.

Greater clearness will be attained if we take a numerical example. Let it be required to run a tunnel through sand, which has an angle of repose $\varphi=23°$, the height and greatest width to be 27 feet each. Required the form of the archway at a depth at the crown=40 feet below surface. The following will be our data:

Height from crown to invert $= h = 27$ ft.
Extreme width . . . 27 ft.
Half span . . . $S = 13.5$ ft
Depth of crown below surface $= x_0 = 40$ ft.
$\varphi = 23_0$

From the above we must obtain the following dimensions:

Depth below surface of greatest horizontal diam $= x_0$
Rise of arch above greatest horizontal diam $= a$
Thickness of brickwork at crown $= t$
Ditto at depth x_1 . . $= t_1$
Radius at crown . . . $= r_0$
Radius at depth x_1 . . . $= r_1$
Hydrostatic half span . . $= y$

The first operation necessary is to find the value of the coefficient C.

$\varphi=23° \therefore \text{Sine } \varphi = .39073$
$1-\text{Sin } \varphi = .60927. \quad 1+\text{Sin } \varphi = 1.39073$

$$\because \sqrt{\frac{1-\text{Sin }\varphi}{1=\text{Sin }\varphi}} = \sqrt{.4381} = .662 = C$$

Now C is the ratio of the

$$\frac{\text{half geostatic span}}{\text{half hydrostatic span}}$$

or $C = \frac{S}{y}$

S has been fixed at 13 ft. 6 in.

$$\therefore y = \frac{13.5}{.662} = 20.4 \text{ ft.}$$

We have now to design a hydrostatic arch with a half span of 20.4 feet, and a crown depth of load of 40 feet, from which, by a horizontal contraction of $\frac{13.5}{20.4}$, we obtain the tunnel we require.

The one dimension we require for this is $x^0 = x_1 + a$, a being the depth of greatest width below the crown, and x_1 below the surface. This dimension a must be obtained by a process of trial and error, the first trial often being sufficient.

The full height h being 27 feet we will approximate to a by making it to equal,

$$h \times \frac{S}{y} = 27 \times \frac{135}{204}$$

or nearly, 18 feet, and we will take it as 18 feet. Then, using Rankine's approximate formula, make

$$b = y + \frac{y^2}{30a} = 20.4 + \frac{416.16}{540} = 21.17$$

this gives one value to b calculated with the factor a as in the denominator. Another value of b is obtained from the formula

$$b = a \sqrt[3]{\left(\frac{x_0 + a}{x_0}\right)} = 18 \sqrt[3]{\frac{58}{40}} =$$

$18 \times 1.13185 = 20.3733$. Here the factor a is in the numerator.

Taking the real value of b as a mean between these two we have

$$b = .5\,(21.17 + 20.3733) = 20.77$$

Now we have also from Rankine a value of $y = b - \frac{b^2}{30a}$ and taking b at 20.77 we may place the formula, thus assuming still that we retain 18 as the value of a

$$y = 20.77 - \frac{431.4}{540} \text{ or } y = 20.77 - .799 =$$

19.97 feet. Now, the true value of y as calculated from c and the proposed half span of the tunnel is 20.4 feet, the result obtained as a first approximation is thus .43 of a foot too narrow.

Multiplying 19.97 by c we get

$19.97 \times .662 = 13.22$, as compared with the intended half span $s = 13$ ft. 6.

a discrepancy of less than three inches and a half on the half span. We may judge from this that the first approximation to the height a of the crown above the greatest horizontal diameter was slightly in error, and that a should be a little over 18 feet, but it is unnecessary in the present instance to obtain a nearer approximation to the exact dimension, because if the half span of the tunnel is

made 13½ feet instead of 13.22, the increased outward thrust of the side walls will be sustained by the resistance of the earth; whereas, if made exactly to theoretical dimensions, there is a greater probability of lateral collapse taking place, and though we may not exactly in theory build to theoretical lines, it is as well to know exactly what these lines are, so that with the knowledge of the strata obtained by experience we may make a just and proper allowance for this contingency, the sidewalls of a tunnel being more liable to fail by crushing inwards than the roof. Thus we see that the greatest diameter is by no means necessarily at the floor level, but the sidewalls incline inwards in a curve below the level of greatest depth, their tendency to slip inwards being resisted by the invert, which it is essential should be well bedded on the substratum, and curved to such a radius that if the material underlying it tends to force it upwards it may resist that tendency equally with the sidewalls' resistance.

A very near approximation to a would be obtained by taking a mean value between $a = h \times c$ and y.

Thus in the present case the true value of a is very nearly 19 feet or

$$\frac{18 + 20.4}{2} = 19.2$$

We will, however, adhere in our drawing to the first approximation, 18 feet, as allowing the above margin for lateral thrust.

It may be as well remarked here that for deeply buried tunnels the profile may be made truly elliptical, no deference being paid to the geostatic form, because the ratio of $x_0 : x_1$ is so nearly unity in such a case, and a circular profile transformed into an ellipse merely be an alteration of the horizontal widths in the ratio $\frac{c}{1}$ is the required section for such an archway.

In designing an elliptic tunnel the half span s is first to be decided upon, and from this the circular radius $y = \frac{s}{c}$ is to be found, then y is also the major axis of the ellipse, of which the tunnel is a portion, just so much of the ellipse being appropriated as is required to give the required height. When the depth of earth above the tunnel crown is very small the ellipse is quite unsuited, and it is important that the geostatic form be considered, for it is evident that if the tunnel crown be only 5 feet from the surface of the ground the value of the expression

$$\sqrt[3]{\left(\frac{x_0 + a}{x_0}\right)}$$

becomes much increased.

With the prevalence of underground railways at small depths this becomes important, and in the tunnel above considered we should get, as the value of a, about 14 feet, or, in other words, the line of greatest width of the tunnel would be but little above its half height.

Hence we see the necessity for strong wiry walls at the mouths of tunnels, and a rapid surchargement of the earth above, so that the necessity of so nearly a circular section may be surmounted for. The profile of the above tunnel would evidently be nearly circular, the greatest horizontal diameter being so nearly at half height, and the side walls below this point sloping inwards with almost the same slope as their inclination above the same point.

For a double track tunnel especially, this would be very inconvenient, necessitating a great amount of ballast filling to give room for the track and vehicles. Above the line of greatest width, however, there would be better accommodation for the upper part of the vehicles than in a tunnel like that shown in Fig. 2, for instance.

When, therefore, it becomes requisite to cut a tunnel very near the surface, it may sometimes be found better to give it a width greater than is absolutely required, in order that the dimension a, being thus increased, the figure of the tunnel may include a smaller part only of that portion below the greatest diameter. Such a correction is less difficult on account of the greater headroom allowed in the more circular archway. In fact, the nearer we approach the surface the more does the figure of the arch approximate to the hydrostatic arch, and the greater is the extra [otherwise unnecessary] width of excavation necessary.

A glance at Fig. 2 will show this, for, supposing the figure of the tunnel were made to accord with the hydrostatic

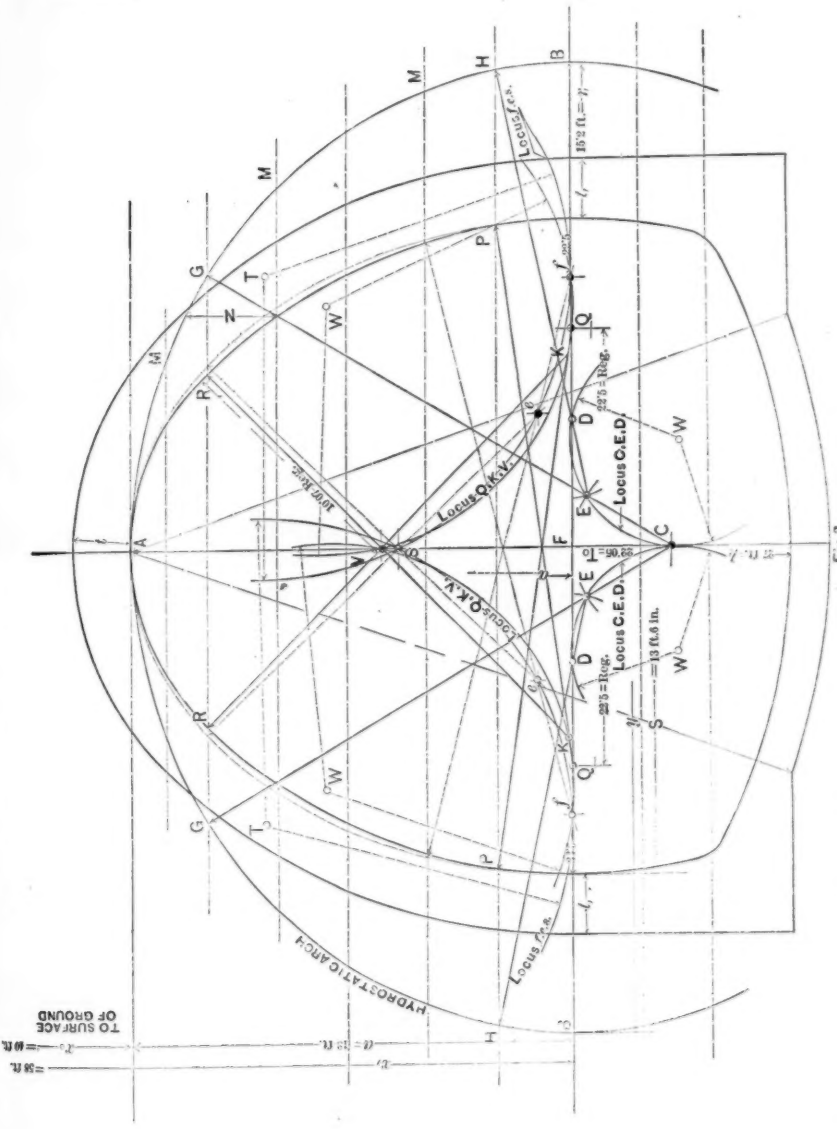


Fig. 2

Locus of Centers, Hydrostatic Arch, C.E.D. Center of Locus W.
" " Geostatic by reduced ordinates *f.e.s.* Curve dotted. Center of Locus T.
" " calculated radii ($x_0^3 x_1$) Q.K.V. Curve in full line. Center of Locus W.

Span 27 ft.
Height 27 ft.

curve it is evident that if the inner arch was calculated to give a sufficiency of headway at the line N, then with the wider arch the floor might be raised by the amount N, and the clearance would still be sufficient, and thus the greater amount of excavation due to width would be in part compensated for by the decreased amount at the floor. It is important that a tunnel section should not at the sides be flatter than the geostatic form would allow, though there appears, as a rule, to be no objection, especially in firm ground, to the curva-

ture being increased, for the outward thrust in this case is taken by the earth behind, whilst in the former case the sides crush or are in danger of crushing inwardly. In fact the outward thrust is in stone arches, as Rankine remarks, specially counted upon the arch tending to spread, laterally compresses the material of the spandrils at the haunches, until the spandril resistance is equal to the thrust of the arch. On this account, therefore, the arch centering of elliptical and similar approximately hydrostatic arches should not be struck until time has been allowed for the settlement and hardening of the material of the spandril filling. Rankine says the backing should be solid up to the level of the crown.

It now remains for us to draw in the figure of the hydrostatic arch, and by transformation the tunnel section, to do which we must find the radius of the hydrostatic arch at the crown and at the greatest horizontal diameter, that is at a depth x_0 and x_0+a, or x_1.

Rankine's formula for this is as follows: Calling r_0 the radius at the crown and r_1 that at the depth x_1, we have

$$r_0=a+\frac{a^2}{2x_0} \text{ and } r_1=a-\frac{a^2}{2(x_0+a)}$$

Substituting known values of a and x_0 we get

$$r_0=18+\frac{18^2}{80}=22.05 \text{ ft.}$$

and $$r_1=18-\frac{18^2}{2\times 58}=15.2 \text{ ft.}$$

to draw in the hydrostatic figure (Fig. 2) the crown radius $r_0=22$ ft. 0.6 inches must be set out from the crown upon the center vertical. The springing radius r_1 =15 ft. 2.4 inches must then be set off from the end B of the greatest horizontal diameter. Then the points C and D thus found are two centers. Then from C, with the radius CF, draw an arc intersecting at E, another arc drawn from D as a center, with a radius equal to FA—BD, *i.e.*, to the difference of the least and greatest radii of the arch. Then CG drawn through E, and EH drawn through D, mark the limits of the arch curve struck from the three centers C, E and D. Having drawn in this curve, the true profile of the tunnel may be found by reducing the breadth of the tunnel at several horizontal lines, as MM in the ratio C to 1.

The radius at the crown and at the greatest diameter may be calculated thus:

The reduced arch crown radius is to the radius of the hydrostatic crown, as $C^2:1$, and at the greatest horizontal diameter the same ratio is $\frac{1}{c}$, the crown radius becoming smaller and the springing radius larger.

Now the value of c was determined from the frictional stability of the earth to be .662, and having fixed our half span as 13 ft. 6, we found our hydrostatic span to be 20 ft. 4.8 inches. Having, however, chosen a somewhat too small we found the width of the arch which resulted come out as 19.97 instead of as 20.4, and as in our drawing of the hydrostatic arch we have made the half span FB=19.97 we must alter the value of c proportionately. The new value is

$$C_c=\frac{20.4\times C}{19.97}=\frac{204\times 662}{1997}=.676$$

and the horizontal ordinates of the tunnel will be less than the corresponding hydrostatic ordinates in the ratio .676 to 1, to which ratio a scale of reduction can be made. Practically the ratio is $\frac{2}{3}$. For the alteration of the radii with .676 $=c_c$ the new value of c, we get $c^2=.457$, and $\frac{1}{c}=1.48$ nearly. Then the geostatic radii at crown and springing r_0g, and r_1g respectively are found to be

$$r_0g=22.05\times.457=10.07,$$
$$r_1g=15.2\ \times 1.48=22\ 5.$$

With these radii the crown and springing curves may be drawn to such limits as are indicated by the intersections of the radii at R and P with the horizontal projection from the limiting points G and H of the hydrostatic radii, as already indicated, the intermediate portion of the arch RP being struck with a radius found by producing the limiting radii at R and P until they intersect at K, which is a point in the locus of centers. The coincidence, or approximate coincidence of the tunnel profiles found by this method and the method of reduced horizontal co-ordinates will show the extent o error or difference in the method of re-

duced ordinates, though both are for all practical purposes sufficiently accurate.

The method of intersection of radii for the intermediate portions of the geostatic arch does not give exactly accurate meeting of the three arcs with the three centers alone, but the crown and springing arcs being struck with the radii as calculated. The remaining portion is to be drawn in by a few trial approximations. The centers of curvature all lying upon the curves marked locus QKV, which curves are drawn from the centers W to pass through the three determined points Q, K and V.

The method given by Rankine of finding the hydrostatic arch by an approximation of the centers CED is stated by him to give a curve too full at G and too contracted at H. This shows itself, in the present case, the method of transformation by reduction of the horizontal ordinates, producing a tunnel section (shown dotted) which falls beyond the section, found by calculated radii, at the level of *c* and the locus of centers *f e s* for striking it approximately, having the centers T in place of W.

It is unnecessary to go into calculations of greater refinement for the purpose of tunnelling, and indeed it may be said that the sections of tunnels were fixed before the subject ever received the attention of mathematicians, these forms being merely empirical alone, and found by experiment, that is, by actual practice, for in the early days of railway engineering the brick linings of tunnels would frequently give way, and when this took place the section would be made of sharper curvature at the point of weakness thus indicated, and in time the tunnel form was correctly developed to be afterwards explained mathematically. There is, however, good reason that engineers should understand the principles underlying buried archways. They are thus enabled to design their structures to approximate to correctness. In studying Rankine, a young engineer is often led to attach undue importance to theoretical forms, especially in regard to stone or brick archways, which are stated to be insecure when the middle third of the arch ring does not contain the curve of equilibrium. Doubtless, for a simple arch ring, this may be very true, but in practice with the solid spandril backing carried up almost to crown level, and with a considerable depth of ballasting and roadbed, it is almost impossible for an arch, as usually constructed, to give way in the manner indicated by Rankine, the thrust being by no means entirely borne by the so-called arch ring, but by the mass of backing or spandril walls also.

The thickness of tunnel walls is usually determined by practical experience, and is rarely less than 18 inches. It should be sufficient to withstand the superincumbent weight of earth which, for each foot of length of tunnel will be

$$w[x_1(S+t)-('8a\times S)],$$

the minus quantity in the equation being the approximate area of the half tunnel above the line of greatest breadth, and w being the weight per cubic foot of the superincumbent earth.

It is only in archways near the surface and in loose earth that it can be assumed that the side walls of a tunnel bear the whole weight of material between the tunnel and the ground surface, and when no other data are as hand it is best to assume a thickness at calculated from the empirical formula generally used for the purpose, namely,

$$t \text{ in feet} = \sqrt{(.12 \text{ to } .5)\times r}$$

according to the firmness or slipperyness of the material, r being taken as $\frac{a^2}{s}$, which in the present case will give 23 feet.

With the coefficient .2 this will give

$$t = \sqrt{.2\times 23} = \sqrt{4.6} \text{ or say 2ft. 3 in.}$$

It is, of course, specially important that as the lining of a tunnel is built in the void between the brickwork or masonry, and the excavated sides of the tunnel be firmly rammed up solid and close, so that when the earth pressure comes upon the tunnel, possibly with greater force at first at some point than at another, the archway may not be able to crush in at that particular point, owing

to the remainder of the arch being unable to yield outwardly by reason of the support of the well rammed earth behind.

It is this precaution in practice which has no doubt contributed to the stability of many rule-of-thumb structures which do not coincide at all closely with the theoretical form as deduced from the principle of the hydrostatic arch.

ELECTRIC LAUNCHES.

By A. RECKENZAUN.

From the "Journal of the Society of Arts."

It is not my intention to treat this subject from a shipwright's point of view. The title of this paper is supposed to indicate a mode of propelling boats by means of electrical energy, and it is to this motive power that I shall have the honor of drawing your attention.

The primary object of a launch, in the modern sense of the word, lies in the conveyance of passengers on rivers and lakes, less than for the transport of heavy goods; therefore, it may not be out of place to consider the conveniences arising from the employment of a motive power which promises to become valuable as time and experience advance. In a recent paper before the British Association at Southport, I referred to numerous experiments made with electric launches; now it is proposed to treat the subject in a wider sense, touching upon the points of convenience in the first place; secondly, upon the cost and method of producing the current of electricity; and thirdly, upon the construction and efficiency of the propelling power and its accessories.

Whether it is for business, pleasure, or war purposes, a launch should be in readiness at all times, without requiring much preparation or attention. The distances to be traversed are seldom very great, fifty to sixty miles being the average.

Nearly the whole space of a launch should be available for the accommodation of passengers, and this is the case with an electrically-propelled launch. We have it on good authority that an electric launch will accommodate nearly double the number of passengers that a steam launch of the same dimensions would; therefore, for any given accommodation we should require a much smaller vessel, demanding less power to propel it at a given rate of speed, costing less, and affording easier management.

A further convenience arising from electromotive power is the absence of combustibles, and the absence of the products of combustion — matters of great importance; and for the milder seasons, when inland navigation is principally enjoyed, the absence of heat, smell, and noise, and, finally, the dispensing with one attendant on board, whose wages, in most cases, amount to as much or more than the cost of fuel, besides the inconvenience of carrying an additional individual.

I do not know whether the cost of motive power is a serious consideration with proprietors of launches, but it is evident that if there be a choice between two methods of equal qualities, the most economical method will gain favor. The motive power on the electric launch is the electric current; we must decide upon the mode of procuring the current. The mode which first suggested itself to Professor Jacobi, in the year 1838, was the primary battery, or the purely chemical process of generating electricity.

Jacobi employed, in the first instance, a Daniell's battery, and in later experiments with his boat on the river Neva, a Grove's battery. The Daniell's battery consisted of 320 cells containing plates of copper and zinc; the speed attained by the boat with this battery did not reach one mile and a quarter per hour; when 64 Grove's cells were substituted, the speed came to two and a quarter miles per hour; the boat was 28ft. long, 7½ft. beam, and 3ft. deep. The electromotor was invented by Professor Jacobi; it virtually consisted of two discs, one of which was stationary, and carried a num-

ber of electro-magnets, while the other disc was provided with pieces of iron serving as armatures to the pole pieces of the electro-magnets, which were attracted whilst the electric current was alternately conveyed through the bobbins by means of a commutator, producing continuous rotation.

We are not informed as to the length of time the batteries were enabled to supply the motor with sufficient current, but we may infer, from the surface of the acting materials in the battery, that the run was rather short; the power of the motor was evidently very small, judging by the limited speed obtained, but the originality of Jacobi deserves comment, and for this, as well as for numerous other researches, his name will be remembered at all times.

It may not be generally known that an electric launch was tried for experimental purposes, on a lake at Penllegaer, near Swansea. Mr. Robert Hunt in the discussion of his paper on electro-magnetism before the Institution of Civil Engineers, in 1858, mentioned that he carried on an extended series of experiments at Falmouth, and at the instigation of Benkhausen, Russian Consul-General, he communicated with Jacobi upon the subject. In the year 1848, at a meeting of the British Association at Swansea, Mr. Hunt was applied to by some gentlemen connected with the copper trade of that part, to make some experiments on the electrical propulsion of vessels; they stated that although electricity might cost thirty times as much as the power obtained from coal, it would, nevertheless, be sufficiently economical to induce its employment for the auxiliary screw ships employed in the copper trade with South America.

The boat at Swansea was partly made under Mr. (now Sir William) Grove's directions, and the engine was worked on the principle of the old toys of Ritchie, which consisted of six radiating poles projecting from a spindle, and rotating between a large electro-magnet. Three persons traveled in Hunt's boat, at the rate of three miles per hour. Eight large Grove's cells were employed, but the expense put it out of question as a practical application.

Had the Gramme or Siemens machine existed at that time no doubt the subject would have been further advanced, for it was not merely the cost of the battery which stood in the way, but the inefficient motor, which returned only a small portion of the power furnished by the zinc.

Professor Silvanus Thompson informs us that an electric boat was constructed by Mr. G. E. Dering, in the year 1856, at Messrs. Searle's yard, on the River Thames; it was worked by a motor in which rotation was effected by magnets arranged within coils, like galvanometer needles, and acted on successively by currents from a battery.

From a recent number of the *Annales de L'Électricité* we learn that Count de Moulins experimented on the lake in the Bois de Boulogne, in the year 1866, with an iron flat-bottomed boat, carrying twelve persons. Twenty Bunsen cells furnished the current to a motor on Froment's principle turning a pair of paddle-wheels.

In all these reports there is a lack of data. We are interested to know what power the motors developed, the time and speed, as well as dimensions and weights.

Until Trouvé's trip on the Seine in 1881, and the launch of the Electricity on the Thames in 1882, very little was known concerning the history of electric navigation.

Mr. Trouvé originally employed Planté's secondary battery, but afterwards reverted to a bichromate battery of his own invention. In all the primary batteries hitherto applied with advantage, zinc has been used as the acting material. Where much power is required, the consumption of zinc amounts to a formidable item; it costs, in quantity, about 3d per pound, and in a well-arranged battery a definite quantity of zinc is transformed. The final effect of this transformation manifests itself in electrical energy, amounting to about 746 watts, or one electrical horse-power for every two pounds of this metal consumed per hour. The cost of the exciting fluid varies, however, considerably; it may be a solution of salts or it may be dilute acid. Considering the zinc by itself, the expense for five electrical or four mechanical horse-power through an efficient motor, in a small launch, would be 2s. 6d. per hour. Many persons would willingly

sacrifice 2s. 6d. per hour for the convenience, but a great item connected with the employment of zinc batteries is in the exciting fluid, and the trouble of preparing the zinc plates frequently. The process of cleaning, amalgamating, and re-filling is so tedious that the use of primary batteries for locomotive purposes is extremely limited. To recharge a Bunsen, Grove, or bichromate battery, capable of giving six or seven hours work at the rate of five electrical horse-power, would involve a good day's work for one man; no doubt he would consider himself entitled to a full day's wages, with the best appliances to assist him in the operation.

Several improved primary batteries have recently been brought out, which promise economical results. If the residual compound of zinc can be utilized and sold at a good price, then the cost of such motive power may be reduced in proportion to the value of those by-products.

For the purpose of comparison let us now employ the man who would otherwise clean and prepare the primary cells at engine-driving. We let him attend to a six horse-power steam-engine, boiler, and dynamo machine for charging 50 accumulators, each of a capacity of 370 ampere hours, or one horse-power hour. The consumption of fuel will probably amount to 40 lbs. per hour, which, at the rate of 18s. a ton, will give an expenditure of nearly 4d. per hour. The energy derived from coal in the accumulator costs, in the case of a supply of 5 electrical horse-power for 7 hours, 2s. 9d.; the energy derived from the zinc in a primary battery, supplying 5 electrical horse-power for seven hours, would cost 17s. 3d.

It is hardly probable that any one would lay down a complete plant, consisting of a steam or gas engine and dynamo, for the sole purpose of charging the boat cells, unless such a boat were in almost daily use, or unless several boats were to be supplied with electrical power from one station. In order that electric launches may prove useful, it will be desirable that charging stations should be established, and on many of the British and Irish rivers and lakes there is abundance of motive power, in the shape of steam or gas-engines, or even water wheels.

A system of hiring accumulators ready for use may, perhaps, best satisfy the conditions imposed in the case of pleasure launches.

It is difficult to compile comparative tables showing the relative expenses for running steam launches, electric launches with secondary batteries, and electric launches with primary zinc batteries; but I have roughly calculated that, for a launch having accommodation for a definite number of passengers, the total costs are as 1, 2.5, and 12, respectively, steam being lowest and zinc batteries highest.

The accumulators are, in this case, charged by a small high-pressure steam-engine, and a very large margin for depreciation and interest on plant is added. The launch taken for this comparison must run during 2,000 hours in the year, and be principally employed in a regular passenger service, police and harbor duties, postal service on the lakes and rivers of foreign countries, and the like.

The subject of secondary batteries has been so ably treated by Professor Silvanus Thompson, and Dr. Oliver Lodge, in this room, that I should vainly attempt to give you a more complete idea of their nature. The improvements which are being made from time to time mostly concern mechanical details, and although important, a description will scarcely prove interesting.

A complete Faure-Sellon-Volkmar cell, such as is used in the existing electric launches is here on the table; this box weighs, when ready for use, 56 lbs.; and it stores energy equal to 1 horse-power for 1 hour=1,980,000 foot-pounds, or about 1 horse-power per minute for each pound weight of material. It is not advantageous to withdraw the whole amount of energy put in; although its charging capacity is as much as 370 ampere hours, we do not use more than 80 per cent., or 300 ampere hours; hence, if we discharge these accumulators at the rate of 40 amperes, we obtain an almost constant current for 7½ hours; one cell gives an E. M. F. of 2 volts. In order to have a constant power of 1 horse for 7½ hours, at the rate of 40 amperes discharge, we must have more than 9 cells per electrical horse-power; and 47 such cells will supply 5 electrical horse-power for the

time stated, and these 47 cells will weigh 2,632 lbs.

We could employ half the number of cells by using them at the rate of 80 amperes, but then they will supply the power for less than half the time. The fact, however, that the cells will give so high a rate of discharge for a few hours, is, in itself, important, since we are enabled to apply great power if desirable; the 47 cells above referred to can be made to give 10 or 12 electrical horse power for over two hours, and thus propel the boat at a very high speed, provided that the motor is adapted to utilize such powerful currents.

The above-mentioned weight of battery power—viz., 2,632 lbs., to which has to be added the weight of the motor and the various fittings—represents, in the case of a steam launch, the weight of coals, steam-boiler, engine and fittings. The electro motor capable of giving four horse-power on the screw shaft need not weigh 400 lbs., if economically designed; this, added to the weight of the accumulators, and allowing a margin for switches and leads, brings the whole apparatus up to about 28 cwt.

An equally powerful launch engine and boiler, together with a maximum stowage of fuel, will weigh about the same. There is, however, this disadvantage about the steam power, that it occupies the most valuable part of the vessel, taking away some eight or nine feet of the widest and most convenient part, and in a launch of 24 feet length, requiring such a power as we have been discussing, this is actually one-third of the total length of the vessel, and one-half of the passenger accommodation; therefore I may safely assert that an electric launch will carry about twice as many people as a steam launch of similar dimensions.

The diagram on the wall represents sections of an electric launch built by Messrs. Yarrow & Co., and fitted up by the Electrical Power Storage Company, for the recent Electrical Exhibition in Vienna. She has made a great number of successful voyages on the River Danube during the autumn. Her hull is of steel, 40 ft. long and 6 ft. beam, and there are seats to accommodate forty adults comfortably. Her accumulators are stowed away under the floor, so is the motor, but owing to the lines of the boat the floor just above the motor is raised a few inches. This motor is a Siemens D_2 machine, capable of working up to 7 horse power with 80 accumulators.

In speaking of the horse-power of an electro-motor, I always mean the actual power developed on the shaft, and not the electrical horse-power; this, therefore, should not be compared to the indicated horse-power of a steam engine.

I am indebted to Messrs. Yarrow for the principal dimensions and other particulars of a high-pressure launch engine and boiler, such as would be suitable for this boat. From these dimensions I prepared a second diagram representing the steam-power, and when placed in a position it will show at a glance how much space this apparatus will occupy. The total length lost in this way amounts to 12 feet, leaving for seating capacity only 15 feet, while that of the electric launch is 27 feet on each side of the boat; thus, the accommodation is as fifteen to twenty-seven, or as twenty-two passengers to forty, in favor of the electric launch.

Comparing the relative weights of the steam power and the electric power for this launch, we find that they are nearly equal, each approaches 50 cwt.; but in the case of the steam launch we include 10 cwt. of coals, which can be stowed into the bunkers, and which allow 15 hours' continuous steaming, whereas the electric energy stored up will only give us seven and a-half hours with perfect safety.

I have here allowed 8 lbs. of coal per indicated horse-power per hour, and 10 horse-power giving off 7 mechanical horse-power on the screw shaft; this is an example of an average launch engine. There are launch engines in existence which do not consume one-half that amount of fuel, but these are so few, so rare, and so expensive, that I have neglected them in this account.

Not many years ago, a steam launch carrying a seven hours' supply of fuel was considered marvelous.

Our present accumulator supplies 33,000 foot-pounds of work per pound of lead, but theoretically one pound of lead manifests an energy equal to 360,000 foot-pounds in the separation from its oxide; and in the case of iron, Prof. Osborne Reynolds told us in this place, the energy evolved by its oxidation is equiva-

lent to 1,900,000 foot-pounds per pound of metal. How nearly these limits may be approached will be the problem of the chemist; to prophesy is dangerous, whilst science and its application are advancing at this rapid rate.

Theoretically then, with our weight of fully oxidized lead, we should be able to travel for 82 hours; with the same weight of iron for 430 hours, or 18 days and nights continually, at the rate of 8 miles per hour, with one charge. Of course, these feats are quite impossible. We might as well dream of getting 5 horse-power out of a steam-engine for one pound of coal per hour.

Whilst the chemist is busy with his researches for substances and combinations which will yield great power with small quantities of material, the engineer assiduously endeavors to reconvert the chemical or electrical energy into mechanical work suitable to the various needs.

To get the maximum amount of work with a minimum amount of weight, and least dimensions combined with the necessary strength, is the province of the mechanical engineer; it is a grand and interesting study; it involves many factors; it is not, as in the steam engine and the hydraulic machine, a matter of pressures, tension and compression, centrifugal and static forces, but it comprises a still larger number of factors, all bearing a definite relation to each other.

With dynamo machines the aim has been to obtain as nearly as possible as much electrical energy out of the machine as has been put in by the prime mover, irrespective of the quantity of material employed in its construction. Dr. J. Hopkinson has not only improved upon the Edison dynamo, and obtained 94 per cent. of the powers applied in the form of electrical energy, but he got 50 horse-power out of the same quantity of iron and copper where Edison could only get 20 horse-power—and, though the efficiency of this generator is perfect, it could not be called an efficient motor, suitable for locomotion by land or water, because it is still too heavy. An efficient motor for locomotion purposes must not only give out in mechanical work as nearly as possible as much as the electrical energy put in, but it must be of small weight, because it has to propel itself along with the vehicle, and every pound weight of the motor represents so many foot-pounds of energy used in its own propulsion; thus, if a motor weighed 660 pounds, and were traveling at the rate of 50 feet per minute, against gravitation, it would expend 33,000 foot-pounds per minute in moving itself, and although this machine may give 2 horse-power, with an efficiency of 90 per cent., it would, in the case of a boat or a tram-car, be termed a wasteful machine. Here we have an all important factor which can be neglected, to a certain extent, in the dynamo as a generator, although from an economical point of view, excessive weight in the dynamo must also be carefully avoided.

The proper test for an electro-motor, therefore, is not merely its efficiency, or the quotient of the mechanical power given out, divided by the electrical energy put in, but also the number of feet it could raise its own weight in a given space of time, with a given current, or, in other words, the number of foot-pounds of work each pound weight of the motor would give out.

The Siemens' D_2 machine, as used in the launch shown in the diagram on the wall is one of the lightest and best motors; it gives 7 horse-power on the shaft, with an expenditure of 9 electrical horse-power, and it weighs 658 lbs.; its efficiency, therefore, is $\frac{7}{9}$ths, or nearly 78 per cent.; but its "co-efficient" as an engine of locomotion is 351—that is to say, each pound weight of the motor will yield 351 foot-pounds on the shaft. We could get even more than 7 horse-power out of this machine, by either running it at an excessive speed, or by using excessive currents; in both cases, however, we should shorten the life of the apparatus.

An electro-motor consists, generally, of two or more electro-magnets so arranged that they continually attract each other, and thereby convey power. As already stated, there are numerous factors, all bearing a certain relationship to each other, and particular rules which hold good in one type of machine will not always answer in another, but the general laws of electricity and magnetism must be observed in all cases. With a given energy expressed in watts, we can arrange a quantity of wire and iron to

produce a certain quantity of work; the smaller the quantity of material employed, and the larger the return for the energy put in, the greater is the total efficiency of the machine.

Powerful electro-magnets, judiciously arranged, must make powerful motors. The ease with which powerful electro-magnets can be constructed, has led many to believe that the power of an electro-motor can be increased almost infinitely, without a corresponding increase of energy spent. The strongest magnet can be produced with an exceedingly small current, if we only wind sufficient wire upon an iron core. An electro-magnet excited by a tiny battery of 10 volts, and, say, one ampere of current may be able to hold a tremendous weight in suspension, although the energy consumed amounts to only 10 watts, or less than $\frac{1}{75}$th of a horse-power; but the suspended weight produces no mechanical work. Mechanical work would only be done if we discontinued the flow of the current, in which case the said weight would drop; if the distance is sufficiently small, the magnet could, by the application of the current from the battery, raise the weight again, and if that operation is repeated many times in a minute, then we could determine the mechanical work performed. Assuming that the weight raised is 1,000 lbs., and that we could make and break the current two hundred times a minute, then the work done by the falling mass could, under no circumstances, equal $\frac{1}{7}\frac{1}{5}$th of a horse-power, or 440 foot-pounds; that is, 1,000 lbs. lifted 2.27 feet high in a minute, or about one-eighth of an inch for each operation; hence the mere statical pull, or power of the magnet, does in no way tend to increase the energy furnished by the battery or generator, for the instant we wish to do work we must have motion —work being the product of mass and distance.

Large sums of money have virtually been thrown away in the endeavor to produce energy, and there are intelligent persons who to this day imagine that, by indefinitely increasing the strength of a magnet, more power may be got out of it than is put in.

Large field-magnets are advantageous, and the tendency in the manufacture of dynamo machines has been to increase the mass of iron, because with long and heavy cores and pole pieces there is a steady magnetism ensured, and therefore a steady current, since large masses of iron take a long time to magnetize and demagnetize; thus very slight irregularities in the speed of an armature are not so easily perceived. In the case of electro-motors these conditions are changed. In the first place, we assume that the current put through the coils of the magnets is continuous; and secondly, we can count upon the momentum of the armature, as well as the momentum of the driven object, to assist us over slight irregularities. With electric launches we are bound to employ a battery current, and battery currents are perfectly continuous—there are no sudden changes; it is consequently a question as to how small a mass of iron we may employ in our dynamo as a motor without sacrificing efficiency. The intensity of the magnetic field must be got by saturating the iron, and the energy being fixed, this saturation determines the limit of the weight of iron. Soft wrought iron, divided into the largest possible number of pieces, will serve our purpose best. The question of strength of materials plays also an important part. We cannot reduce the quantity and division to such a point that the rigidity and equilibrium of the whole structure is in any way endangered.

The armature, for instance, must not give way to the centrifugal forces imposed upon it, nor should the field magnets be so flexible as to yield to the statical pull of the magnetic poles. The compass of this paper does not permit of a detailed discussion of the essential points to be observed in the construction of electro-motors; a reference to the main points may, however, be useful. The designer has, first of all, to determine the most effective positions of the purely electrical and magnetic parts; secondly, compactness and simplicity in details; thirdly, easy access to such parts as are subject to wear and adjustment; and, fourthly, the cost of materials and labor. The internal resistance of the motor should be proportioned to the resistances of the generator, and the conductors leading from the generator to the receiver.

The insulation resistances must be as high as possible; the insulation can

never be too good. The motor should be made to run at that speed at which it gives the greatest power with a high efficiency, without heating to a degree which would damage the insulating material.

Before fixing a motor in its final position, it should also be tested for power with a dynamometer, and for this purpose a Prony brake answers very well.

An ammeter inserted in the circuit will show at a glance what current is passing at any particular speed, and voltmeter readings are taken at the terminals of the machine, when the same is standing still as well as when the armature is running, because the E M.F. indicated when the armature is at rest alone determines the commercial efficiency of the motor, whereas the E.M.F. developed during motion varies with the speed until it nearly reaches the E.M.F in the leads; at that point the theoretical efficiency will be highest.

Calculations are greatly facilitated, and the value of tests can be ascertained quickly, if the constant of the brake is ascertained; then it will be simply necessary to multiply the number of revolutions and the weight at the end of the lever by such a constant, and the product gives the horse-power, because, with a given Prony brake, the only variable quantities are the weight and the speed. All the observations, electrical and mechanical, are made simultaneously. The electrical horse-power put into the motor is found by the well known formulæ $C \times E \div 746$; this simple multiplication and division becomes very tedious and even laborious if many tests have to be made in quick succession, and to obviate this trouble, and prevent errors, I have constructed a horse-power diagram.

Graphic representations are of the greatest value in all comparative tests. Mr. Gisbert Kapp has recently published a useful curve in the *Electrician*, by means of which one can easily compare the power and efficiency at a glance.

The speeds are plotted as abscissæ, and the electrical work absorbed in watts divided by 746 as ordinates; then with a series-wound motor we obtain a curve. The shape of this curve depends on the type of the motor. Variation of speed is obtained by loading the brake with different weights. We begin with an excess of weight which holds the motor fast, and then a maximum current will flow through it without producing any external work. When we remove the brake altogether, the motor will run with a maximum speed, and again produce no external work, but in this case very little current will pass. Between these two extremes external work will be done, and there is a speed at which this is a maximum.

I have now to draw your attention to a new motor of my own invention, of the weight of 124 lbs., which, at 1,550 revolutions, gives 31 amperes and 61.5 volts at terminals. The mechanical horse-power is 1.37, and the co-efficient 373.

	Ohms.
Armature resistance....	.4*w*.
Field-magnet resistance..	.17*w*.
Insulation resistance....	1,500,000*w*.

This motor was only completed on the morning before reading the paper; it could not, therefore, be tested as to its various capacities.

We have next to consider the principle of applying the motive power to the propulsion of a launch. The propellers hitherto practically applied in steam navigation are the paddle wheel and the screw. The experience of modern steam navigation points to the exclusive use and advantage of the screw propeller where great speed of shaft is obtainable, and the electric engine is pre-eminently a high-speed engine, consequently the screw appears to be most suitable to the requirements of electric boats. By simply fixing the propeller to the prolonged motor shaft, we complete the whole system, which, when correctly made, will do its duty in perfect order, with an efficiency approaching theory to a high degree.

Whatever force may be imparted to the water by a propeller, such force can be resolved into two elements, one of which is parallel, and the other in a plane at right angles to the keel. The parallel force alone has the propelling effect; the screw, therefore, should always be so constructed that its surfaces shall be chiefly employed in driving the water in a direction parallel to the keel from stem to stern.

It is evident that a finely-pitched screw, running at a high velocity, will supply

these conditions best. With that beautiful screw lying on this table, and made by Messrs. Yarrow, 95 per cent. of efficiency has been obtained when running at a speed of over 800 revolutions per minute, that is to say, only 5 per cent. was lost in slip.

Reviewing the various points of advantage, it appears that electricity will, in times to come, be largely used for propelling launches, and, perhaps, something more than launches.

In conclusion, quoting Dr. Lardner's remarks on the subject of steam navigation of nearly fifty years ago, he said:

"Some, who, being conversant with the actual conditions of steam engineering as applied to navigation, and aware of various commercial conditions which must affect the problem, were enabled to estimate calmly and dispassionately the difficulties and drawbacks, as well as the disadvantages, of the undertaking, entertained doubts which clouded the brightness of their hopes, and warned the commercial world against the indulgence of too sanguine anticipations, of the immediate and unqualified realization of the project. They counseled caution and reserve against an improvident investment of extensive capital, in schemes which can still be only regarded as experimental, and which might prove its grave. But the voice of remonstrance was drowned amid the enthusiasm excited by the promise of an immediate practical realization of a scheme so grand.

"It cannot," he continues, "be seriously imagined that any one who had been conversant with the past history of steam navigation could entertain the least doubt of the abstract practicability of a steam vessel making the voyage between Bristol and New York. A steam vessel, having as cargo a couple of hundred tons of coals, would, *cæteris paribus*, be as capable of crossing the Atlantic as a vessel transporting the same weight of any other cargo."

Dr. Lardner is generally credited with having asserted that a steam voyage across the Atlantic was "a physical impossibility," but in the work from which I took the liberty of copying his words he denies the charge, and says that what he did affirm was, that long sea voyages could not at that time be maintained with that regularity and certainty which are indispensable to commercial success, by any revenue which could be expected from traffic alone.

The practical results are well-known to us. History repeats itself, and the next generation may put on record our weak attempts, our doubts and fears of this day. Whether electricity will ever rival steam, remains yet to be proved; We may be on the threshold of great things. The premature enthusiasm has subsided, and we enter upon the road of steady progress.

ELECTRICAL UNITS OF MEASUREMENT.

By Sir WILLIAM THOMSON, F.R.S., M. Inst. C. E.

From the Proceedings of the Institution of Civil Engineers.

In physical science a first essential step in the direction of learning any subject, is to find principles of numerical reckoning, and methods for practicably measuring, some quality connected with it. I often say that when you can measure what you are speaking about, and express it in numbers, you know something about it; but when you cannot measure it, when you can not express it in numbers, your knowledge is of a meagre and unsatisfactory kind: it may be the beginning of knowledge, but you have scarcely, in your thoughts, advanced to the stage of *science*, whatever the matter may be. I may illustrate by a case in which this first step has not been taken. The hardness of different solids, as precious stones and metals, is reckoned by a merely comparative test. Diamond cuts ruby, ruby cuts quartz, quartz, I believe, cuts glass-hard steel, and glass-hard steel cuts glass; hence, diamond is reckoned harder than ruby; ruby, than quartz; quartz, than glass-hard steel; and glass-hard steel than glass; but we have no nu-

merical measure of the hardness of these, or of any other solids. We have, indeed, no knowledge of the moduluses of rigidity, or of the tensile strength, of almost any of the gems or minerals, of which the hardness is reckoned by mineralogists in their comparative scale, beginning with diamond, the hardest of known solids. We have even no reason to believe that the modulus of rigidity of diamond is greater than that of other solids; and we have no exact understanding of what this property of hardness is, nor of how it is related to moduluses of elasticity, or to tensile or shearing strength, or to the quality of the substance in respect to its bearing stresses exceeding the limit of its elasticity. It must, therefore, be admitted that the science of strength of materials, so all-important in engineering, is but little advanced; and the part of it relating to the so-called hardness of different solids least of all; there being in it no step toward quantitative measurement or reckoning in terms of a definite unit.

A similar confession might have been made regarding electric science, as studied even in the chief physical laboratories of the world ten years ago. True, Cavendish and Coulomb, last century, and Ampere and Poisson, and Green, and Gauss, and Weber, and Ohm, and Lentz, and Faraday, and Joule, this century, had given us the mathematical and experimental foundation for a complete system of numerical reckoning in electricity and magnetism, in electro-chemistry, and in electro-thermodynamics; and as early as 1858 a practical beginning of definite electric measurement had been made, in the testing of copper resistances, insulation resistances, and electro-static inductive capacities, of submarine cables. But fifteen years passed after this beginning was made, and resistance coils and ohms, and standard condensers and micro-farads, had been for ten years familiar to the electricians of the submarine-cable factories and testing stations, before anything that could be called electric measurement had come to be regularly practiced in most of the scientific laboratories of the world. I doubt whether ten years ago a single scientific instrument maker or seller could have told his customers whether the specific conductivity of his galvanometer coils was anything within 60 per cent. of that of pure copper; and I doubt whether the resistances of one in a hundred of the coils of electro-magnets, galvanometers, and other electro-magnetic apparatus, in the universities, and laboratories, and lecture establishments of the world, were known to the learned professors whose duty it was to explain their properties, and to teach their use to students and pupils. But we have changed all that; and now we know the resistances of our electro-magnetic coils, generally speaking, better than we know their lengths, and our least advanced students in physical laboratories are quite able to measure resistances through a somewhat wide range with considerable accuracy. I should think, indeed, that with the appliances in ordinary use they are more likely to measure resistances of from 100 to 10,000 ohms to an accuracy of $\frac{1}{10}$ per cent. than they are to be right to 1 millimeter in a meter in their measurements of length. It certainly is a very surprising result, that in such a recondite phenomenon—such a subtle quality to deal with—as electric resistance, which is so very difficult to define, and which we are going to learn is a velocity, every clerk in a telegraph station, the junior students and assistants in laboratories, and even workmen in electric lighting establishments, are perfectly ready to measure, more accurately than you would measure the length of 10 feet of wire, the resistance of electric conductors in definite absolute units.

I suppose, too, that nearly every apparatus room and physical laboratory possesses a micro-farad, but I am afraid its pedigree is not often known; and if its accuracy within 10 per cent. were challenged, I doubt whether, in many cases, any one, whether maker or possessor, or other electrical expert, could be found to defend it. As for our electrostatic apparatus, I confess that I do not know the capacity of a single one of the two or three dozen Leyden jars which, in 1846, I inherited in the Natural Philosophy apparatus room of the University of Glasgow, or which I have made from time to time during the thirty-seven years passed since that date. I would fain hope that I am singular in such a confession, and that no other professor of natural phil-

osophy in the world would let a Leyden jar be put on his lecture-room table without being able to tell his students its capacity in absolute measure. The reckoning of Leyden jar capacity in square inches of coated glass—thickness and specific inductive capacity not stated—ought to be as much a thing of the past as is the reckoning of resistances in terms of a mile of wire—weighing 14 grains to the foot, of ordinary commercial copper, specific resistance not stated (perhaps 45 per cent.? or 70 per cent.? or 98 per cent? of the conductivity of pure copper). And as to practical measurement of electro-motive force, we have scarcely emerged one year from those middle ages when a volt and a Daniell's cell were considered practically identical—to the higher aspiration of measurement within 1 per cent. It seems, indeed, as if the commercial requirements of the application of electricity to lighting, and other uses of every-day life, were destined to cause an advance of the practical science of electric measurement, not less important and valuable in the higher region of scientific investigation, than that which, from twenty to thirty years ago, was brought about by the practical requirements of submarine telegraphy.

There cannot be a greater mistake than that of looking superciliously upon practical applications of science. The life and soul of science is its practical application, and just as the great advances in mathematics have been made through the desire of discovering the solution of problems which were of a highly practical kind in mathematical science, so in physical science many of the greatest advances that have been made, from the beginning of the world to the present time, have been made in the earnest desire to turn the knowledge of the properties of matter to some purpose useful to mankind.

The first step toward numerical reckoning of properties of matter more advanced than the mere reference to a set of numbered standards as in the mineralogist's scale of hardness, or to an arbitrary trade standard, as in the Birmingham wire gauge, is the discovery of a continuously-varying action of some kind, and the means of observing it definitely, and measuring it in terms of some arbitrary unit or scale division. But more is necessary to complete the science of measurement in any department; and that is the fixing on something absolutely definite as the unit of reckoning, which, with reference to electric and magnetic science, is the subject of my lecture of this evening.

In electricity the mathematical theory and the measurements of Cavendish, and in magnetism, the measurements of Coulomb gave, one hundred years ago, the requisite foundation for a complete system of measurement: and fifty years ago the same thing was done for electro-magnetism by Ampere.

I speak of electricity, of magnetism, and of electro-magnetism. Now, I must premise, as a matter of importance in respect of some of the technical details which we shall have to consider a little later, that magnetism must be held to include electro-magnetism. Electro-magnetism and magnetism are one and the same thing. Electro-magnetic and electro-static force, which are very distinct just now, are two things which deeper science may lead us to unite, in a manner that we can scarcely see at present. We have the foundation in the last century of Cavendish for electricity, and of Coulomb for magnetism, which falls in perfectly with what I shall have to say a little later on in respect of Gauss and Weber's work, of magnetism and electromagetism. I say this, because there has been some little discussion in respect to the magnetic unit and the electro-magnetic unit, as if the magnetic unit might be something different from the electro-magnetic unit, or the electro-kinetic unit. It will simplify matters if we think merely of a magnetic force, whether it be due to a steel magnet, or to a wire conveying a current, and make no distinction so far as measurement is concerned, through the range of the science of magnetism, including electro-magnetism. We shall find that we have the two capital subjects: electricity and electro-static force, one of them; magnetism and electricity and motion through conductors, and magnetic and electro-magnetic force, the other. The first complete method of scientific measurement for any of these subjects was that of Gauss, in his system of absolute measurement for terrestrial magnetism so splendidly realized by Gauss and Weber in their Magnetic So-

ciety of Göttingen, which gave the starting impulse for the whole system of absolute measurement as we now have it, throughout the range of electric science. In fact, Weber himself, after realizing absolute measure in terrestrial magnetism in conjunction with Gauss, carried it on through the field of electro-magnetism in his "Elektrodynamische Maasbestimmungen," and thence into electro-statics in his joint work with Kohlrausch, under the same title, "Elektrodynamische Maasbestimmungen." The now celebrated "v" (velocity) which Maxwell, in his electro-magnetic theory of light, pointed out to be not merely by chance approximately equal to the velocity of light, but to be probably connected physically, in virtue of the forces concerned, with the actual action or motion of matter which constitutes light, was found to be approximately 300,000 kilometers per second.

As early as 1851 I commenced using the absolute system in the reckoning of electromotive forces of voltaic cells, and the electric resistances of conductors, in absolute electro-magnetic units; and after advocating the general use of the absolute system, both for scientific investigation and for telegraph work, for ten years, I obtained in 1861 the appointment of a Committee of the British Association on Electrical Standards.

This committee worked for nearly another ten years through the whole field of electro-magnetic and electro-static measurement, but chiefly on standards of electric resistance, until in its final report, presented to the Exeter meeting in August, 1869, it fairly launched the absolute system for general use; with arrangements for the supply of standards for resistance coils in terms of a unit, first called the British Association unit, and afterwards the Ohm; of which the resistance reckoned in electro-magnetic measure was to be, as nearly as possible, 10,000 kilometers per second.

In regard to the name of "ohm," I may mention that a paper was communicated to the British Association in 1861 by Sir Charles Bright and Mr. Latimer Clark, in which the names that we now have, with some slight differences, were suggested; and a complete continuous system of measurement was proposed, which did not fulfill certainly all the conditions of the absolute system, but which fulfilled some of them in an exceedingly useful manner for practical purposes. To Sir Charles Bright and Mr. Latimer Clark, therefore, is due the whole system of names as we have it now, ohms, volts, farads, and micro-farads. From 1870 or 1871 forward, the absolute system, with the approach to accurate realization of it given by the British Association unit, has been in general use in England and America; but another decade has passed, a rather long one, before the definitive practical adoption of the absolute system by France, Germany and other European countries, as decreed by the International Conference, for the determination of Electric Units, held at Paris in October 1882. The decision adopted was, not to take the British Association unit. Doubt was thrown upon its accuracy, which we shall see was well founded. The question of a strict foundation for a metrical system was before the conference, and it was inclined to adopt the absolute system, but the question occurred, "What is the ohm?" Who can see an ohm? Who can show what an ohm is? Who can measure the resistance of any conductor for us, in this absolute measure of Weber's? Weber's own measurement differed greatly from that of the British Association. Several experimenters, in endeavoring to verify or test the British Association measurement, arrived at results which were discordant among themselves, and therefore could not be confirmatory of the British Association measurement. Things were in this doubtful state, and the conference had a very important practical question to decide. A proposal had been before the world for ten years at least, to found accurate measurement of electrical resistance upon a material obtainable in uniform quality and by easy precautions in a state of perfect purity, or sufficiently nearly perfect to fit it practically for the purpose in question, which is—the giving of a standard for the measurement of resistance. The Siemens unit, founded upon the specific resistance of mercury, had been proposed. The great house of Siemens (Berlin and London) our distinguished *confrere*, Sir William Siemens, and his distinguished brother, Dr. Werner Siemens, worked upon this subject in the most thorough and powerful way—the measurement of resistances in terms of the

specific resistances of mercury—in such a manner as to give us a standard which shall be reproducible at any time and place, with no other instrument of measurement at hand than the meter measure. I say, the system of measurement of resistance on a mercury standard had been worked out, and its practicability demonstrated. Werner and William Siemens themselves were both present at the conference, and they joined heartily in the proposal to adopt an absolute system, but the question was how to make a beginning; and the answer adopted by the conference was to ask for a definition of an absolute system in terms of a column of mercury. The column of mercury was the one standard in existence, that could be reproduced otherwise than by merely copying from one wire to another; and it was naturally adopted as the foundation upon which a standard, if not a practical unit to be used, should be founded. In short, then, the finding of the conference was to this effect: that as soon as good evidence is given of a sufficiently near measurement for practical purposes, of the resistance of any conductor—be it a piece of wire or a column of mercury—as soon as such measurement should be made, with evidence that it is accurate enough for practical purposes, then the unit which the British Association had aimed at should be adopted; but it was to be left to the judgment and the convenience of the users of standards when to make the change, should a change be necessary, from the British Association unit as the ohm, or from the Siemens unit, to bring measurement into more close agreement with the absolute reckoning. What had been done by Lord Rayleigh and Mrs. Sidgwick, had left very little room for doubt but that the British Association unit was in error to the extent of 1.3 per cent. The Siemens unit had the advantage of being somewhat approximately equal to the desired absolute unit, though not professing to be an absolute unit at all. It was simply the resistance of a column of mercury at zero temperature, a meter in length and a square millimeter in section. There were great difficulties in the reproduction of the Siemens unit, in the earlier times of the investigation; but Dr. Werner Siemens, and Lord Rayleigh, and Mrs. Sidgwick, and many other workers, besides, all working to compare with the British Association unit, obtained results which finally left no doubt whatever as to the true relation. Dr. Werner Siemens' result found the mercury unit to be 0.9536 of the British Association unit; Lord Rayleigh and Mrs. Sidgwick found it 0.9542, which is an exceedingly close agreement, being within $\frac{1}{10}$ per cent. of the result of Dr. Werner Siemens. A result differing by nearly 1 per cent. had been obtained by Mathiessen and Hockin a good many years before, when the precautions necessary to reproduce the mercury standard with absolute accuracy, were not so well known as, in the course of a few years after their work, they came to be known. The final conclusion of Lord Rayleigh's work was, that the Siemens mercury unit is 0.9413, of what the conference at Paris agreed to define as the ohm; and that is the resistance measured by 1,000,000,000 centimeters per second. I am afraid that conveys a strange idea, but it is perfectly true as to the absolutely definite meaning of resistance. I shall have occasion to refer to the subject later, when I hope to explain this mysterious velocity of 10_9 centimeters per second. In the course of the thirty years from the time when telegraphy began to demand definite measurement, a great deal of accurate measurement in terms of variously defined units of resistance had been made. Many sets of resistance coils had been produced by the Varley brothers, and other instrument makers, and many scientific investigators in laboratories had produced standards, and sets of resistance coils were made according to those standards; but within the last twelve years all have merged into, either the Siemens, or the British Association unit. The British Association unit, as I have said, was an attempt at absolute measurement, which succeeded in coming within 1.3 per cent. of the 10_9 aimed at. *Copies* of the British Association unit were *accurate to* $\frac{1}{10}$ *per cent.* The Siemens unit was founded on another idea, but it gave results no less definite and no less convenient for a great multitude of practical applications, than did the somewhat nearer approach to a convenient absolute unit realized by the British Association Committee.

Gauss' principle of absolute measure-

ment for magnetism and electricity, is merely an extension of the astronomer's method of reckoning mass in terms of what we may call the universal-gravitation unit of matter; and of the reckoning of force adopted by astronomers, in common with all workers in mathematical dynamics, according to which the unit of force is that force, which, acting on unit of mass for unit of time, generates a velocity equal to unit of velocity. The universal-gravitation unit of mass is such a quantity of matter, that if two quantities, each equal to it, be placed at unit distance apart, the force between them is unity.

The universal-gravitation method I refer to for this reason. There is a terrestrial-gravitation reckoning of force, according to the weight of the unit of mass; and after all, when we terrestrial creatures take a mass in our hand and feel the weight of it, it is a kind of measurement that we cannot do away with. The kilogramme, or the pound, or the ounce, is a thing we have to deal with; we have it in our hand, and we cannot help using it to give us by its *heaviness* a reckoning of force. A local gravitation unit of force means the weight of a gramme in London, in Glasgow, at the Equator, or anywhere else; and it is a convenient unit; but the common mode of measuring force by reference to weight without reference to locality is not definite, because the weight of a gramme is different here from what it is at the Equator. The heaviness of a pound or a gramme is greater by a two-hundredth at either pole than at the Equator; or to give the exact figures, 0.00512. That is a difference of $\frac{1}{2}$ per cent., and if your accuracy is to be within a $\frac{1}{2}$ per cent., you cannot ignore the difference of the force of gravity in different places. But a vast number of measurements in engineering, and in teh most ultra scientificwork of scientific laboratories, does not aspire to so high a degree of accuracy; and for all such work the local or terrestrial-gravitation unit suffices, without specifying what the particular place is—only that it is somewhere or other on the face of the earth. For instance, moduluses of rigidity, moduluses of rupture, breaking strains of material, are stated accurately enough for engineering purposes, in terms of a ton weight per square centimeter, or pounds weight per square centimeter, or any other such mode of reckoning; or if I had not vowed never to mention inches, I would say tons per square inch, which is common (perhaps too common) in engineering. All such measurements ignore the difference of gravity in different localities, except some more precise measurements, in which an allowance for the force of gravity to reduce it to a standard of lat. 45° is made, or it is left to the person using the measurement to make the reduction. For all purposes, however, in whieh it would be desirable to apply a correction for the varying force of gravity in different places, it is convenient to use Gauss' absolute unit, and not the terrestrial-gravitation unit of force. I may say in passing, that the mere idea, which lurked or was visibly manifested, according to the degree of understanding, in the old formula of elementary dynamics $F=m\frac{dv}{dt}$, was an immense step; and the realization of that idea, the bringing of it into practical use, has contributed more than anything else I know, to the intelligent treatment of the dynamic problems and their applications to both scientific and engineering matters. The system of absolute reckoning of force by Gauss cannot be too much commended, as a great and important practical improvement in the fundamental science of engineering and physics, the science of dynamics. It consists simply in defining the unit of force as that force which, acting on a unit of mass for a unit of time, generates a velocity equal to the unit of velocity. It leaves the units of mass, space, and time to be assumed arbitrarily; the gramme, the centimeter, and the mean solar second, for example, as in the now generally adopted "C. G. S." system.

But the universal-gravitation system of the dynamical astronomer defines the unit of mass in terms of the unit of space and the unit of force. I need not repeat the definition. Thus we have the interlocking of two definitions: the unit of force defined in terms of the units of mass, space, and time; the unit of mass defined in terms of the unit of force and the unit of space. It might seem as if we were proceeding in a vicious circle; but the circle is not vicious—the two definitions are logically and clearly inter-

dependent. We have, as it were, two unknown quantities and two equations; and the elimination of one of the unknown quantities from the two equations, gives us the other explicitly. The two are mixed up in a somewhat embarrassing way in the primitive definitions, but when we disentagle them, we arrive at the simple result, which I shall state presently, of independent definitions of the unit of mass and the unit of force, each in terms of units of space and time chosen arbitrarily.

Though the units of force and mass thus defined are essentially implied in all the regular formulas of physical astronomy from those most elementary ones, which appear in the treatment of the undisturbed elliptic motion, according to Newton's inferences from Kepler's laws, up to the most elaborate working out of the lunar, planetary, and cometary theories, and the precession and nutation of the earth's axis; it has not been usual for physical astronomers to found any systematic numerical reckoning upon them nor even to choose arbitrarily and definitely any particular units of length and time on which to found the units of force and mass. It is, nevertheless, interesting, not only in respect to the ultimate philosophy of metrical systems, but also as full of suggestions regarding the properties of matter to work out in detail the idea of founding the measurements of mass and force on no other foundation than the measurement of space and time. In doing so we immediately find that the square of an angular velocity, is the proper measure of density or mass per unit volume; and that the fourth power of a linear velocity is the proper measure of a force. The first of these statements is readily understood by referring to Clerk Maxwell's suggestion, of taking the period of revolution of a satellite revolving in a circle close to the surface of a fixed globe of density equal to the maximum density of water, as a fundamental unit for the reckoning of time. Modify this by the independent adoption of a unit of time, and we have in it the foundation of a measurement of density, with the detail that the density of the globe is equal to $3/(4\pi)$ of the square of the satellite's angular velocity in radius* per second: that is, the square of the satellite's velocity, multiplied by 3 and divided by 4π, measures the density of the globe. It may be a hard idea to accept, but the harder it is the more it is worth thinking of, and the more instructive in regard to the properties of matter. There it is, explain it how you will, that the density of water, the density of brass, the mean density of the earth, is measured absolutely in terms of the square of an angular velocity. I do not know whether it is generally known that to Fourier are due those dimensional equations that appear in the British Association's volume of reports, and in Clerk Maxwell's book, and in Everett's useful book, "Units and Physical Constants." The dimension for the reckoning of density is the square of an angular velocity on the universal gravitation absolute system, and is therefore T^{-2}. Equally puzzling and curious is a velocity to the fourth power for the reckoning of force, which we have next to consider.

The universal gravitation reckoning of force, which we shall see is by the fourth power of a linear velocity, may be explained as follows: Find the velocity with which a particle of matter must be projected to revolve in a circle around an equal particle fixed at such a distance from as to attract it with a force equal to the given force. The fourth power of this velocity is the number which measures the force. Sixteen times the force will give double the velocity; eighty-one times the force will give three times the velocity, and so on.

Now, if I were to say that the weight of that piece of chalk is the fourth power of twenty miles an hour, I should be considered fit, not for this place, but for a place where people who have lost their senses are taken care of. I suppose almost every one present would think it simple idiocy if I were to say that the weight of that piece of chalk is the fourth power of seven or eight yards per hour, yet it would be perfectly good sense.

*The radian is the unit in which angular velocity is expressed. It is an angle of $\left(\frac{180°}{\pi}\right)$ about 57°.3 (or more correctly 56°.2958). Thus an arm, or radius vector turning through an angle of about 57°.3 per second, is moving with unit angular velocity; or if the arm makes a complete circle in one second its angular velocity is 2π.

Think now of an infinitesimal satellite revolving around the earth—you ask, what is an infinitesimal satellite? To be "infinitesimal" for our present purpose it must be very small in comparison with the earth, so as not to cause sensible motion by its reaction on the earth. Well, a 500-lb. shot is an infinitesimal satellite, though it is not, perhaps, infinitesimal in some of its aspects. There must be no resistance of the air, of course. Now fire it off with such a velocity that it will have a very flat trajectory, neither more nor less flat than the earth, and it will continue going round and round the earth. Find the velocity at which you must fire off the shot to make it go round the earth, and, if there is no resistance of the air, there is our infinitesimal satellite. These somewhat pedantic words are justified, because "infinitesimal satellite" is nine syllables to express three or four sentences; that is our justification.

The semi-period of an infinitesimal satellite revolving round the earth, close to its surface, is equal to the semi-period of an ideal simple pendulum of length equal to the earth's radius, and having its weighted end infinitely near to its surface; and, therefore, when reckoned in seconds, is approximately equal to the square root of the number of meters (6,370,000) in the earth's radius; because the length of a seconds pendulum (or the pendulum whose semi-period is a second) is very approximately 1 meter. Thus we find 2,524 mean solar seconds for the semi-period of the satellite, and its angular velocity in radians per second is, therefore $(\pi/2524=)0.001244$; hence the earth's mean density, reckoned on the universal-gravitation system, with the mean solar second for the unit of time, is $[(0.001244)^2\times 3/(4\pi)=]3.70\times 10^{-7}$; and, if we take (from Bailey's repetition of Cavendish's experiment)[2] the earth's mean density as 5.67 times the maximum density of water, we find 6.53×10^{-8} for the maximum density of water according to the universal gravitation reckoning. To measure mass we must now introduce a unit of length, and if we take this as 1 centimeter, we find that as the mass of a cubic centimeter of water at maximum density is very approximately equal to what is called a gramme, the universal gravitation unit of matter is $[1/(6.53\times 10^{-8})=]15.3\times 10^{8}$ grammes, or 15.3 French tons; hence, the unit force on the universal-gravitation system is 15.3×10^{8} dynes, or 15.6 times the terrestrial weight of a kilogramme.

15.3 French tons, then (a French ton is 1.4 per cent. less than the British ton), is the universal-gravitation unit of matter. The time may come when the universal-gravitation system will be the system of reckoning; when 15.3 tons will be the unit of matter, and when the decimal subdivision of 15.3 French tons may be our metrical system, and grammes may be as much a thing of the past as grains are now.

There is something exceedingly interesting in seeing that we can practically found a metrical system on a unit of length and a unit of time. There is nothing new in it, since it has been known from the time of Newton, but it is still a subject full of fresh interest. The very thought of such a thing is full of many lessons in science that have scarcely yet been realized, especially as to the ultimate properties of matter. The gramme, it will be remembered, is founded on the properties of a certain body, namely, water; but here, without invoking any particular kind of matter, simply choosing a certain definite length marked on a measuring rod, and a unit of time (how obtained we shall consider presently), we can take up a piece of matter and tell, in any part of the universe, how to measure its mass in definite absolute units.

Think now of the two units on which this universal-gravitation metrical system depends, the unit of length and the unit of time. The unit of length is merely the length of a certain definite piece of brass, or other solid substance used for a measuring rod, or the length between two marks upon it; it may be an inch, or a foot, or a yard, or a meter, or a centimeter, the principle is the same. The meter, it is true, was made originally as nearly as possible equal to the ten-millionth of the length of a certain quadrant of the earth, estimated as accurately as possible from the geodetic operations of MM. Méchain and Delambre, in 1792, performed for the foundation of the metrical system. But this merely gave the original meter measure, and what is meant by the meter now is a length equal to it, or to some authentic

copy of it, which has been made from it as accurately as possible; and the one-hundredth part of the meter thus defined is the centimeter which we definitively adopt as the unit of length.

Thus our unit of length is independent of the earth, and is perfectly portable, so that the scientific traveler, roaming over the universe, carries his measuring rod with him, and need think no more of the earth so far as his measurement of space is concerned. But how about the mean solar second, in terms of which he measures his time? What of it if he has left the earth for good; or if, even without leaving the earth, he carries on his scientific work on the earth through a few million years, in the course of which the period of the earth's rotation around its axis, and its revolution around the sun, will both be very different from what they are now? If he takes a good watch or chronometer with him, well rated before he leaves the earth, it will serve his purpose as long as it lasts. What it does is merely to count the vibrations of a certain mass under the influence of a certain spring (the balance wheel under the influence of the hair spring). If, for any secular experiment he has in hand, he wishes to keep up a continuous reckoning of time, he must keep his watch always going, and not a vibration will be lost in the counting performed by the hands. But if he merely wishes to keep his unit of time, and to make quite sure that any number of million years hence, this shall be within one-tenth per cent. of its present value, he should take a vibrator better arranged for permanence and for absolute accuracy than the balance wheel with its hair spring of a watch or a chronometer. A steel tuning-fork, which has had its period of vibration determined for him before he leaves the earth, by Prof. Macleod or by Lord Rayleigh, will serve his purpose. By measuring the period in terms of mean solar seconds, with the prongs up, and horizontal, and vertically down, he will be able to eliminate the slight effect of terrestrial gravity; and he will have with him a time standard that will give him the mean solar second, as accurately as his measuring rod gives him the centimeter, in whatever part of the universe, and at whatever time, now or millions of years later, he has occasion to use his instruments.

I hope that you will not feel that I am abusing your good nature with an elaborate frivolity when I ask you to think a little more of the unital equipment of our ideal traveler on a scientific tour through the universe. For myself, what seems the shortest and surest way to reach the philosophy of measurement—an understanding of what we mean by measurement, and which is essential to the intelligent practice of the mere art of measuring—is to cut off all connection with the earth, and think what we must then do to make measurements which shall be definitely comparable with those which we now actually make, in our terrestrial workshops and laboratories. Suppose, then, the traveler to have lost his watch and his tuning-fork and his measuring rod, but to have kept his scientific books, or at all events to have in his mind a full recollection and understanding of their contents: how is he to recover his centimeter, and his mean solar second?

Let us consider the recovery of the centimeter first. Wherever he is let him make a piece of glass, like this which I hold in my hand, out of materials which he is sure to find, in whatever habitable region of the universe he may chance to be; and let him with a diamond, or with a piece of hard steel, or with a piece of flint, engrave on it one thousand equidistant parallel lines, upon a space which may be about the breadth of his thumb, and which he may take as a temporary or provisional unit of length. He may help himself to engrave the glass by means of a screw cut in brass or steel, which he will easily make, though he has no tools, not even flint implements, to begin with. With a little time and perseverance he will make the requisite tools. Let him also make a temporary measuring rod, and mark off equal divisions upon it, which may be of any convenient length, and need not have any relation to the definite provisional unit. Let him now make two candles, and light them and place them as you now see those on the table, at any convenient distance apart, measured on his measuring rod. He holds the piece of ruled glass in his hand, close to his eyes, as I hold this, and sees two rows of colored spectrums, each with one of the candles in its center. He turns the glass around till the two rows of spectrums are in the same line, and

adjusts the parallelism of its plane, so as to make the distance from spectrum to spectrum a minimum. He moves backwards and forwards as I do now, keeping his eye at equal distances from the two candles, until he sees each candle shooting up out of the yellow middle of a spectrum of the other candle, with no spectrum between the two candles. With this condition fulfilled he measures the distance from the grating to the candles. Then, by the theory of diffraction he has the proportion: as the distance from the grating to the candles is to the distance between the candles, so is the distance from center to center of the divisions on the glass, to the wave length of yellow light. This, he remembers, is 5.892×10^{-5} of a centimeter, and thus he finds the value in centimeters of his provisional unit.

[How easily this determination might be effected, supposing the grating once made, was illustrated by a rapid experiment performed in the course of the lecture; without other apparatus than a little piece of glass with two hundred and fifty fine parallel lines engraved on it, two candles, and a measuring tape of unknown divisions of length (used only to measure the ratio between two distances). The result showed the distance from center to center of consecutive bars of the grating, to be 32 times the wavelength of yellow light. The breadth of the span on which the two hundred and fifty lines of grating were ruled, was thus measured as ($250 \times 32 \times 5.892 \times 10^{-5} =$) 0.47136 centimeter. According to the instrument maker this space was said to be 0.5 of a centimeter.]

Thus you see, by this hurried experiment with this rough-and-ready apparatus, we have been able to measure a length to within a small percentage of accuracy. A few minutes longer spent upon the experiment, and using sodium flames behind fine slits instead of open candles blowing about in the air, with more careful measurement of the ratio of the distances, might easily have given a result within one-half per cent. of accuracy. Thus the cosmic traveler can easily recover his centimeter and his meter measure.

But how is our scientific traveler to recover his mean solar second, supposing he has lost his tuning-fork? He may think of the velocity of light, and go through Foucault's experiment. That is a thing that can be done from the beginning, with nothing but cutting tools and pieces of metal to begin with. Let him get a piece of brass and make a wheel, and cut it to two thousand teeth. I do not know how many teeth Foucault used, but our traveler can go through the whole process, and set the wheel revolving at some uniform rate (not a known rate, because he has no reckoning of time): and he will tell what the velocity of the wheel is in terms of the velocity of light, which is known to be about 300,000 kilometers per second. If he is electrically minded, as this evening we are bound to suppose our scientific traveler to be, he will think of "v" or of an ohm. He may make a Siemens unit; that he can do, because he has his centimeter, and he finds mercury and glass everywhere. Then he goes through all that Lord Rayleigh and Mrs Sidgwick have done. He will, with a temporary chronometer or vibrator, obtain a provisional reckoning of time, and he will go through the whole process of measuring the resistance of a Siemens unit in absolute measure, according to his provisional unit of time. His measurement gives him a velocity in, let us say, kilometers per this provisional unit of time, as the value of the Siemens unit in absolute measure. Then he knows from Lord Rayleigh and Mrs. Sidgwick, that the Siemens unit in absolute measure is 9,413 kilometers per mean solar second; and thus he finds the precise ratio of his provisional unit of time to the mean solar second.

Still, even though this method might be chosen as the readiest and most accurate, according to present knowledge of the fundamental data, for recovering the mean solar second, the method by "v" is too interesting and too instructive, in respect to elimination of the properties of matter from our ultimate metrical foundations, to be unconsidered. One very simple way of experimentally determining "v." is derivable from an important suggestion of Clark and Bright's paper, referred to above. Take a Leyden jar, or other condenser of moderate capacity (for example, in electro-static measure, about 1,000 centimeters), which must be accurately measured. Arrange a mechanism to charge it to an accurately measured potential of

moderate amount (for example, in electro-static measure, about 10 c.g.s., which is about 3,000 volts), and discharge it through a galvanometer coil at frequent regular intervals (for example, ten times per any convenient unit of time). This will give an intermittent current of known average strength (in the example, 10° electro-static c.g.s., or about 1/300,000 c.g.s. electro-magnetic, or 1/30,000 of an ampere), which is to be measured in electro-magnetic measure by an ordinary galvanometer. The number found by dividing the electro-static reckoning of the current, by the experimentally found electro-magnetic reckoning of the same, is "*v*," in centimeters per the arbitrary unit of time, which the experimenter in search of the mean solar second has used in his electro-static and electro-magnetic details. The unit of mass which he has chosen, also arbitrarily, disappears from the resulting ratio.

But there is another exceedingly interesting way—a way which, although I do not say it is the most practical, has very great interest attached to it, as being a way of doing the thing in one process—that is, by the method of electrical oscillations. I should certainly like to see how a person who has lost his standards, after having recovered his centimeter (which he certainly would do by the wave-length of light), would succeed in recovering his unit of time by the following method. Take a condenser—a very large Leyden jar; electrify it, and connect the two poles through a conductor, arranged to have as large an electro-magnetic *quasi* inertia, electro-magnetic self-induction—as possible. The method is given in Clerk Maxwell's "Electricity and Magnetism" (vol. ii. chap. xix.): it is too long to explain the details. Read the mathematical parts of Clerk Maxwell, read the British Association volume of Reports on Electrical Standards, and read Everett's "Units and Physical Constants"; get these off by heart from the first word to the last, and you will learn with far less labor than by listening to me. Take a resistance coil of proper form for maximum electro-magnetic inertia, and discharge the condenser through it; or rather start the condenser to discharge through such a coil, and you will have a set of oscillations, following exactly the same law as the oscillations of the water-level in two cisterns, which, having initially had the free water-level in one higher than in the other, are suddenly connected by a U-tube. Imagine two cisterns of water, connected by a U-tube with a stop-cock, and having the water higher in one cistern than in the other; now suddenly open the stop-cock, and the water-level will begin to fall in one cistern, and rise in the other. The inertia of the water, thus made to flow through the connecting U-tube, will cause it to flow on after it has come to its mean level in the two cisterns, and rise to a higher level in the one in which it was previously higher, and sink to a correspondingly low level in the other. Thus the water-level in each cistern would alternately be above and below the mean free level: the range of motion being gradually diminished, in virtue of the viscosity of the water, until after a dozen or two of oscillations, the amplitude of each becomes so small that you cannot notice it. Precisely the same thing happens in the case of the discharge of a condenser through a resistance coil of large electro-magnetic inertia: the resistance of the copper wire being like the viscous influence which causes the oscillations of water to subside. If, in his investigations throughout the universe, our traveler could meet with a metal which is about a million times as conductive as copper, he would make this experiment with much greater ease; but it is practicable with copper. It is certain from the observations made by Feddersen, Schiller, and others, that a great number of oscillations can be observed, and that the period, or semi-period of oscillation, can be determined with considerable accuracy.

If our scientific traveler wishes, by this beautiful experiment, once for all to determine his time reckoning, let him proceed thus. Let him take a coil, of which he knows the dimensions perfectly, having already gone through the preliminary process of measuring its electrical dimensions; or if he cannot measure these with sufficient accuracy (and there is enormous difficulty in finding the electric dimensional qualities of a coil by measurement), let him do it partly by direct measurement of its length and of linear dimensions of the figure into which it is wound, and partly by comparing it electro-magnetically with other coils. By

an elaborate investigation he can find the electro-magnetic inertia of the coil in terms of his centimeter. And here, again, there is a curious kind of puzzle and apparent incongruity, when I say that the electro-magnetic inertia equivalent of a coil, is a length, and is measured as a numeric of centimeters. Let him make a condenser, and by building it up from small to large, let him learn the capacity of it in electro-static measure. Let him begin with two plates or cylinders, or a sphere enclosed within concentric sphere, and go on multiplying till he gets a capacious enough condenser of which he knows, in electro-static measure, the electro-static capacity. This, again, is a line. Now let him take the rectangle of those two lines, and construct the equivalent square—let him, geometrically or arithmetically, take the square root of the product of the two lines—and let him observe the period of electric oscillation that I have spoken of. Let him imagine the hand of a watch, going once round in the observed period. He has good magnetic eyes, and he sees the electro-magnetic oscillation, or he has appliances by which he can test it: the thing has been done. He sets in motion a little piece of wheel-work, with a hand going once round in the period of the oscillation. Now, for a moment, let him imagine that hand to be equal in length to the square root of the product of those two lines—several million centimeters, or several thousand kilometers, if the coil and condenser are of dimensions convenient for the actual experiment, as we terrestrials might do it. The velocity of the end of that hand is "v." There he has this wonderful quantity "v." He has a hand going round in a certain time, and he knows that if that hand be of the calculated length, the velocity of the end of it is "v." This is interesting and instructive, and though I do not for certain know that it is very practicable, it is still, I believe, sufficiently so to be worth thinking of. I think it will be one of the ways of determining this marvelous quantity "v."

It is to be hoped that before long "v" will be known, in centimeters per mean solar second, within 1/10 per cent. At present it is only known that it does not *probably* differ 3 per cent. from 2.9×10^{10} centimeters per mean solar second. When it is known with satisfactory accuracy, an experimenter provided with a centimeter measure may, anywhere in the universe, rate his experimental chronometer to mean solar seconds, by the mere electro-static and electro-magnetic operations described above, without any reference to the sun or other natural chronometer.

I have tried your patience, I fear, too long, but I have now only reached the threshold of my subject. We now must commence the consideration of electrical units of measurement. I need not go round defining quantities electro-statically and electro-magnetically; you will find it all in Everett, and in the British Association volume of collected Reports by the first Committee on Electric Measurement. It is not for me to tell you of an ohm, a volt, a micro-farad, and so on; but there are two or three points that I should like to notice, and one is, the limitation of the so-called practical system. The absolute system goes from beginning to end in a perfectly consistent manner, with the initial conditions carried out all through; one of which, in the electro-magnetic system, is that the electromotive force produced by the motion at unit speed, across the lines of force of a field of unit intensity, of a unit length of conductor, is unity. That you must carry out if the system is to be complete and consistent, and the dimensions of all your instruments and apparatus must all be all reckoned uniformly in terms of the unit of length adopted in the absolute definition. The ohm is 1,000,000,000 centimeters, or 10,000 kilometers, per second. If we are to make the ohm an absolute electro-magnetic unit with the second as the unit of time, we must take the earth's quadrant as the unit of length. If we take that consistently throughout, we need never leave this particular system, and we need have nothing to do with C. G. S. We should have the Q. G. S. system, pure and simple! But it would be obviously inconvenient to measure the dimensions of instruments, the diameters of wheels, and the gauges of wire in submultiples of the earth's quadrant. Imagine the horror of a practical workman, on hearing a scientific person say to him, "Give me a wire 1/1,000,000 of an earth-quadrant long, and 1/10,000,000,000 in diameter." Now wherein does the so-called practical sys-

tem differ from the absolute system, and why is it not to be as logical and complete as the absolute system? We would never leave the absolute system, if it gave us in all cases convenient numbers; and it does give us convenient numbers for the measurement of a current, its unit being ten times the "ampere" of the practical system. The unit of resistance in C. G. S., however, is too small, so is the unit of electromotive force. To get convenient numbers, we give names to certain multiples of units, that is all; and we use these multiples just as long as it is convenient, and not any longer. That is my idea of the practical system—to use it for convenience and as long as it is convenient; the moment it ceases to be convenient, to throw it overboard and take C. G. S. pure and simple. The conference at Paris decided upon the practical system, by adopting the units which are now so familiar, the ohm, the volt (taken from the British Association recommendation), and the ampere. The coulomb was also added, and it was most satisfactory to get old Coulomb's name in—one of the fathers of electrical science. Then the watt was added by Sir W. Siemens, and it has been generally accepted, and has proved exceedingly convenient. But when you go farther with the practical system, and take anything that involves a magnetic pole or a magnetic field, you get lost in the trouble of adopting the earth's quadrant as unit of length, and the deviation from C. G. S. ceases to be convenient. Return then to C. G. S. pure and simple.

I spoke of the resistance of an ohm being measured in terms of a velocity. I should like to explain this in a few words. Imagine a mouse-mill set with its axis vertical. Put a pair of brushes at the tops and bottoms of the bars; put the brushes in the magnetic north and south plane through the axis, and set the mouse-mill to spin at any rate you please. Take a galvanometer like a tangent galvanometer, but with only an arc equal to the radius—an arc subtending an angle equal to about 57.3°—and electrodes perpendicular to the plane of the arc connected with the brushes. The mouse-mill must be placed so far from the galvanometer, as not sensibly to influence it. Now take the galvanometer, and turn the mouse-mill; let the length of each bar of the mouse-mill be a centimeter; but that would be a flea-mill rather than a mouse-mill—say, let each bar be 100 centimeters; turn the mouse-mill round fast enough to cause your galvanometer to be deflected 45°. Then one hundred times the velocity of the bars is equal to the resistance in the circuit. Double resistance requires double velocity; half resistance requires half velocity to give the prescribed 45° deflection. There, then, is the rationale of 10,000 kilometers per second, or 1,000,000,000 centimeters per second being the measure of resistance. While we thus measure resistance in electro-magnetic measure by a velocity, we measure a conductivity in electro-statics by a velocity. I have given a very simple explanation of this also in a statement quoted by Sir William Siemens in his presidential address to the British Association at Southampton in 1882. The velocity at which the surface of a globe must shrink towards the center, to keep its potential constant, when it is connected to the earth by a wet thread, measures the conducting power of that wet thread. Double conducting power will require double velocity of shrinkage, that is, the globe must shrink twice as fast not to lose its potential. With a very long semi-dry thread the globe may shrink slowly. Suppose we have a globe insulated in the air of this room for electrical experiment, and connected with the ground by a silk thread. If you have an electrometer to show the potential, you will see it gradually sink. You might imagine that dust in the air would carry off electricity, but in truth practically the sole loss is by this semi dry silk thread. When you see the potential sinking, imagine you see the globe shrinking slowly, so as to keep its potential constant, while it is gradually losing its electric charge little by little: the velocity with which the surface must shrink towards the center to keep the potential constant, measures the conducting power of the thread in electro-static measure. Thus we learn how it is a velocity which measures in electro-static measure the conducting power of a certain thread or wire. But, as we have seen in electro-magnetic measure, the resistance of the same thread or wire is measured by another velocity. The mysterious quantity "v"

is the square root of the product of the two velocities. Or it is the one velocity which measures in electro-magnetic measure the resistance, and in electro-static measure the conductivity, of one and the same conductor: which must be of about 29 ohms resistance, because experiment has proved "v" to be not very different from 290,000 kilometers per second.

I have spoken to you of how much we owe to Sir Charles Bright and Mr. Latimer-Clark for the suggestion of names. How much we owe for the possession of names, is best illustrated by how much we lose—how great a disadvantage we are put to—in cases in which we have not names. We want a name for the reciprocal of resistance. We have the name "conductivity," but we want a name for the unit of conductivity. I made a box of resistance coils thirty years ago, and another fifteen years ago, for the measurement of conductivity, and they both languished for the want of a name. My own pupils will go on using the resistance box in ohms, rather than the conductivity box, because it is so puzzling to say, "The conductivity is the reciprocal of the sum of the reciprocals of these resistances." It is the conductivity that you want to measure but the idea is too puzzling; and yet for some cases the conductivity system is immensely superior in accuracy and convenience to that by adding resistances in series. For the reciprocal of an ohm in the measurement of resisting power—for the unit reckoning of conductivity which will agree with the ohm—it is suggested to take a phonograph and turn it backwards, and see what it will make of the word "ohm." I admire the suggestion, and I wish some one would take the responsibility of adopting it; we should then have *mho* boxes of coils at once in general use. With respect to electric light, what is it we want to measure by the current galvanometer? We have a potential galvanometer, and we have a current galvanometer. Everybody knows what we want to measure with the potential galvanometer. The servant in every house that is lighted electrically knows about potentials; and if in reading the galvanometer he sees it is down to 80 volts he knows that something is wrong, and will at once go to the engine-room and cause 84 volts to be supplied; supposing, for example (as in the case of my own house, temporarily, until I can get two-hundred-volt lamps), that the proper potential is 84 volts. But in the current galvanometer there are so many divisions indicating, it may be, the number of amperes in the current. But after all, what do we want besides a knowledge of the potential? It is the sum of the reciprocals of the resistances in the circuit. In the multiple-arc system each fresh lamp lighted adds a conductivity. In a circuit of Edison or Swan hundred-volt lamps, in each of which you have a current of 0.7 of an ampere, and therefore a resistance of 143 ohms, how convenient it would be, in putting on a lamp—adding a certain conductivity—if we could say we add a *mho*, or a fraction of a *mho*, as the case may be. I do not say that *mho* is the word to be used, but I wish it could be accepted, so that we might have it at once in general use. We shall have a word for it when we have the thing, or rather I should say, we shall have the thing when we have the word. The Appendix to the 1862 Report of the first British Association Committee on Electric Measurements contains a description of a "Resistance Measurer" invented by Sir William Siemens, and a "Modification of Siemens' Resistance Measurer," by Professor Jenkin. This instrument gives directly the resistance of a conductor, by means of an instrumental adjustment, bringing a magnetic needle to a zero position for each observation. In the original Siemens instrument the adjustment is a shifting of two coils by translational motion, and the conductivity is read on a scale of equal divisions adapted, by means of a curve determined by experiment, to give a reading of the required resistance. In Jenkin's modification the mechanical arrangement is much simplified by the adoption of a different electro-magnetic combination; and the required resistance is given by the tangent of the angle through which the coils must be turned to bring the needle to zero. A similar instrument to give conductivity by a simple reading, without any adjusting or "setting" for each observation, is easily made. I made such an instrument in 1858, being simply a galvanometer with controlling resistance coils instead of controlling magnets. Such an instru-

ment at once gives conductivity, and you want a name (suppose you adopt *mho*) for the unit of conductivity, and call the instrument a mhometer. The rule would be, the reciprocal of the sum of the reciprocals of mhos, is equal to the number of ohms; and the reciprocal of the sum of the reciprocals of ohms, is equal to the number of mhos. The number of mhos, or of millimhos, will then measure the number of lamps in the circuit. The domestic incandescent lamp of the early future ought to be, and we hope will be, a one-millimho lamp, to give a 10 or 12-candle light with the Board of Trade regulated 200 volts of potential. Thus the lamp-galvanometer, or lamp-meter, may have its scale divided to one millimho to the division, and the number read on its scale at any time will be simply the number of lamps lighted at the time. The instrument will also have the great advantage of being steady, notwithstanding the variations of the engine. A potential instrument on an electro-light circuit at best is always somewhat variable, because the potential varies a good deal—within 1 or 2 per cent. perhaps—but the resistance in the lamps varies exceedingly little. The mho-meter will in these circumstances be an absolutely steady instrument; you will not see it quiver, even though the engine is irregular. The potential galvanometer will show you how much unsteadiness there is to be complained of or to be corrected.

Lastly, as to the objects to be aimed at in respect to the use of this great system of units. Nothing can be much more satisfactory than is the measurement of somewhat large resistances, as we have it habitually at present; but if we want a little more method for low resistances, it will be helped very much by the use of the mho boxes of conductivities which I have indicated. The great thing we want now in the way of practical electric measurement, is a good standard of electromotive force. That was the chief object of a recent British Association Committee, but it has not yet been satisfactorily attained for practical purposes. Standard cells serve for the purpose to some extent, but we want something better, something of the nature of an electro-dynamometer, to give a good steady idiostatic potential gauge, by which the constant of any electrometer or ordinary galvanometer may be easily and accurately tested. That is an object to be sought; there are plenty of ways of attaining it, and I hope, before another year has passed, to see it realized in many ways, certainly in one way.

As to the science of electricity, the great want in the way of measurement just now is the accurate measurement of ("v") the ratio between the electro-static and the electro-magnetic units; and I hope that scientific investigators will take the matter up, and give to it an accuracy like that which Lord Rayleigh has given to the measurement of the ohm.

A most interesting point remains. It is Joule's work, reported on by the British Association Committee: see volume of Reports on Electrical Standards, p. 138. It was only in my preparation for this lecture that I came upon it, and put the figures definitely together. Joule, with a modesty characteristic of the man, and with a magical accuracy characteristic of his work, made, at the request of the British Association, an investigation of the heating effect of a measured current in a definite way, according to the measure of resistance of the British Association ohm, supposed then to be 10, C. G. S. units of resistance; and he himself considered that the electrical measurement which he then made, was more accurate than his old frictional measurement of the mechanical equivalent of the thermal unit could be. The result obtained, assuming the British Association ohm to be absolutely correct, gave the mechanical equivalent as 782.2 foot-pounds, instead of 772 which he had made it before, and he expressed himself willing to make a new determination of it by the frictional method. But now let us put ourselves in the position of 1866, the date of this report, with these competing determinations of the ohm: that obtained by the British Association method of spinning coils; and by Joule's electro-thermal method with the dynamical value of the thermal unit, as was given by his frictional method. Supposing that his electro-thermal method was right, then what we are to infer is not that the result is the mechanical equivalent, but that the British Association unit was not 10⁹, as it was supposed to

be, but $10^{s} \times 0.98699$. Thus this experiment was virtually Joule's determination of the resistance of the British Association ohm in absolute measure. Lord Rayleigh's determination is $10^{s} \times 0.98677$, a difference of 2 in the 4th place, within about 1/50 per cent. There is perfect magic in the accuracy of Joule's work; it is not a matter of chance. I think between Joule, Lord Rayleigh, Mrs. Sidgwick, and others, we cannot have much doubt now, what is the absolute value of the Siemens unit, or of the British Association unit. I advise everybody to take the Rayleigh ohm unit, instead of the British Association ohm. I have begun to do so, and I mark everything R. O. You may have everything in the British Association unit, but reduce, if you please, to Rayleigh ohms by the reducing factor 0.98677. Volts must be reduced in the same ratio. The old estimate which I made in 1851 from Joule's experiment, for the absolute electromotive force of a standard Daniell cell, was 1.07 volts; and after thinking it was 1.078 for ten years, because of the British Association unit, we come back to correct it, and find it is 1.07. So much for the volt. But we want far more accurate instruments and methods connected with other parts of electric measurement, especially electromotive force and capacity, electro-static, or electro-magnetic, with the comparing number "*v*."

These are the things we want to advance and perfect, to give a satisfactorily scientific character, to this great system of absolute measurement, of which I have endeavored to trace and explain the origin.

TESTING MACHINES, THEIR HISTORY, CONSTRUCTION AND USE.

By ARTHUR V. ABBOTT.

Written for VAN NOSTRAND'S ENGINEERING MAGAZINE.

III.

SPECIAL MACHINES.

Passing now to our second general subdivision of testing machines, we come to the consideration of those which have been designed for special purposes, and perhaps the first and most notable among these is the autographic torsion machine of Prof. Thurston. Fig. 21 is a perspective view of this machine, and in Fig. 22 a front elevation, a longitudinal section on line *aa*, and a transverse section on line *aa'* may be seen. The machine consists of two A-shaped frames AA, AA, and shown in all the figures. These are firmly mounted on a heavy bed-plate. Near the top of each of these frames are spindles C and D, each of which has a rod E and H, with a jaw to receive and hold the heads of the specimens. The two spindles are not connected in any way to each other, except by the specimen itself when placed and adjusted to be tested. To the spindle a long arm HB is attached. It carries a heavy weight B attached to its outward end. This spindle has a norm wheel driven by a worm F on the shaft LL, which is turned by a hand-crank K. When the specimen is placed in the two jaws, and the spindle is turned by the worm gear, the effect is to twist the specimen. The position of the arm and the weight will at all times be a measure of the torsional stress which is exerted on the specimen; but as this torsional stress is increased the specimen commences to give way or be twisted by the stress, according to the quality of the material. In making such torsional tests it is essential that the amount of twist should be known. If a record of this could be procured, it would be an indication of the capacity of the material to resist such stresses, or, in other words, of its quality. This testing machine was designed for this purpose. The record is made in the following way: To the spindle C a cylindrical drum is attached, which is covered with a sheet of cross-section paper. To the arm N a

FIG. 21.

pencil holder is attached at F, and carries a pencil D, the point of which bears upon the paper of the drum. Supposing that the specimen in the machine should offer no resistance, and should merely twist, the pencil would then remain stationary, and as the drum is revolved the pencil would trace a straight line on the paper, the length of which would measure the amount which the specimen was twisted. If, on the other hand, the specimen be supposed to resist and to twist simultaneously, as is always the case, it will be seen that the spindle B would be turned, and the arm N would be moved from a vertical position to a distance proportionate to the stress resisted by the specimen. The pencil holder attached to the arm would move with it. To make a record of this distance a guide curve FF′ is attached to the frame of the machine, so that when the arm N and the pencil holder are moved out of their vertical position the pencil is moved towards the left by the guide curve, which is of such a form that the lateral movement which it gives to the pencil is proportional to the moment of the weight on the end

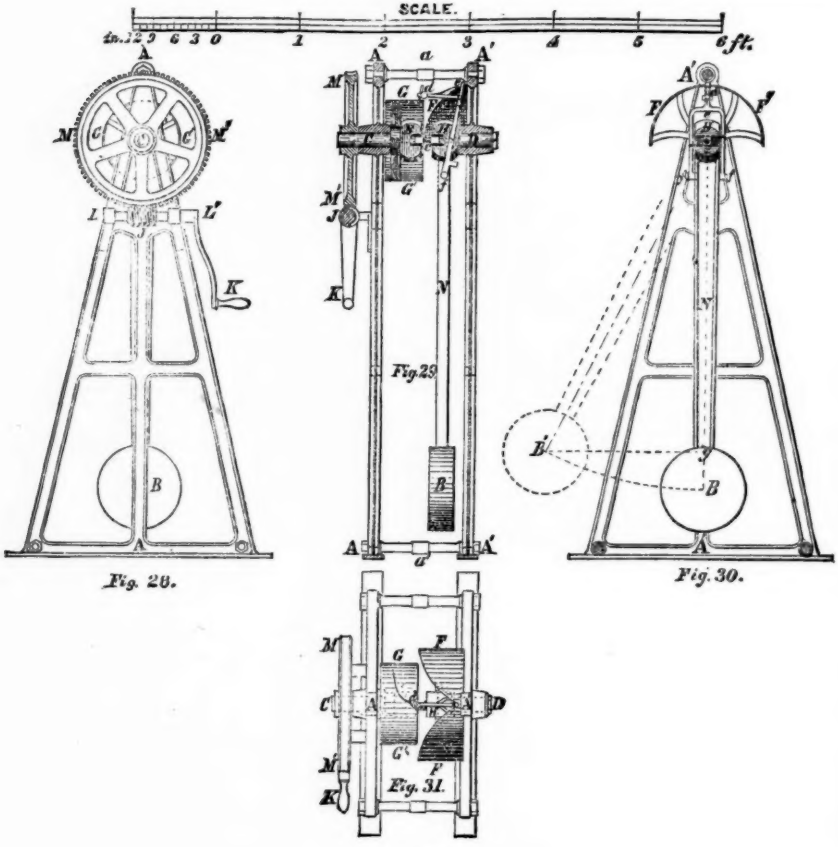


FIG. 22.

of the arm. In this machine each inch of ordinate denotes 100 foot-pounds of moment transmitted through the test piece, and each inch of abscissa ten degrees fo torsion. By use of this machine the metal tested is compelled to tell its own story, and to give a permanent record of its strength, elasticity, and every other quality which is brought into play during the test.

The transverse testing machine of the Stevens Institute, Fig. 23, built by Messrs. Fairbanks & Co., consists of a heavy platform scale, on the platform of which stands a cast-iron frame, to which are fastened the supports *dd* at the required distance apart. The pressure on the specimen is applied by means of a hand wheel on the upper end of the screw *k*, which screw passes from the nut *e*, and terminates in the sliding cross-head *i*. This cross-head serves both as a guide and as a pressure block. The test piece *l*, rests upon mandrils mounted upon the supports *dd*, at the required distance apart. All the loads are weighed in the usual manner at *m*.

A second example of transverse testing machine given in the illustration Fig. 24, is one which is especially designed to meet the requirements of makers and users of cast iron. The construction of the machine is exceedingly simple, and may be almost seen at a glance. A wooden framework supports a hand-wheel for applying the stress to the

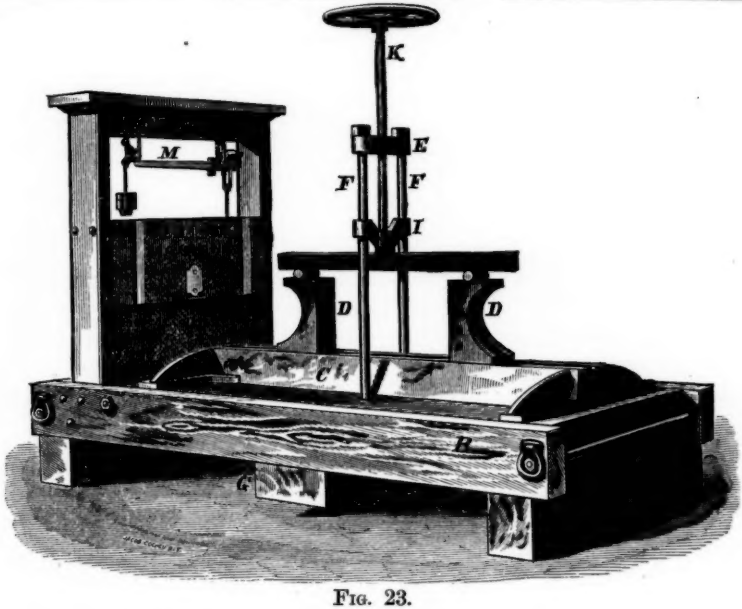


FIG. 23.

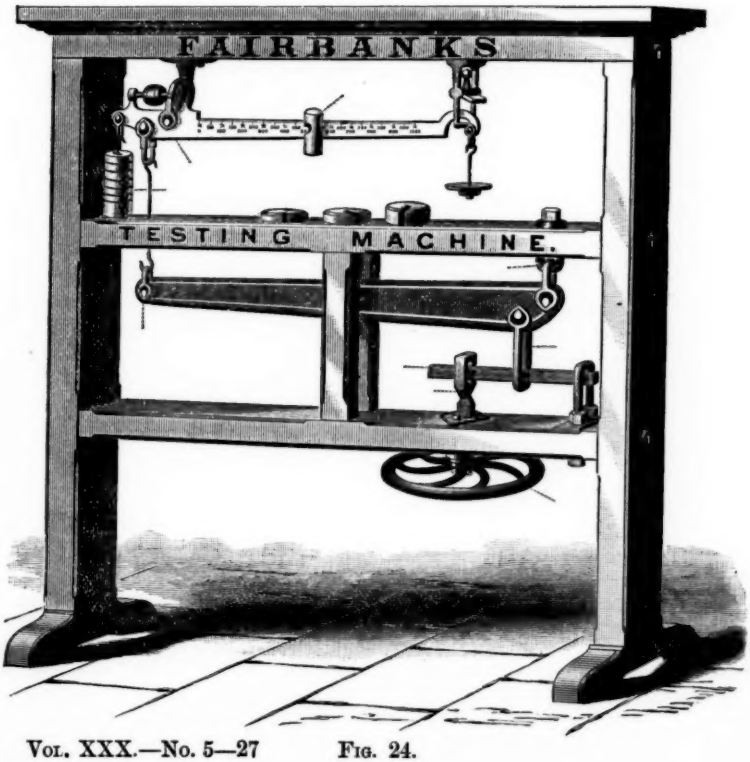


 FIG. 24.

specimen, which is hung from the end of a lever supported close to the wheel. The end of this lever carries the stress to the weighing beam, where it may be estimated by sliding the poise to and fro, and adding the weights to the weight counterpoise.

the scale work of the machine. Perhaps its operation may be clearly understood by describing the method of making the experiment. The cement being mixed with the necessary quantity of water, is pressed into the moulds M, and having stood a sufficient time to set, the mould

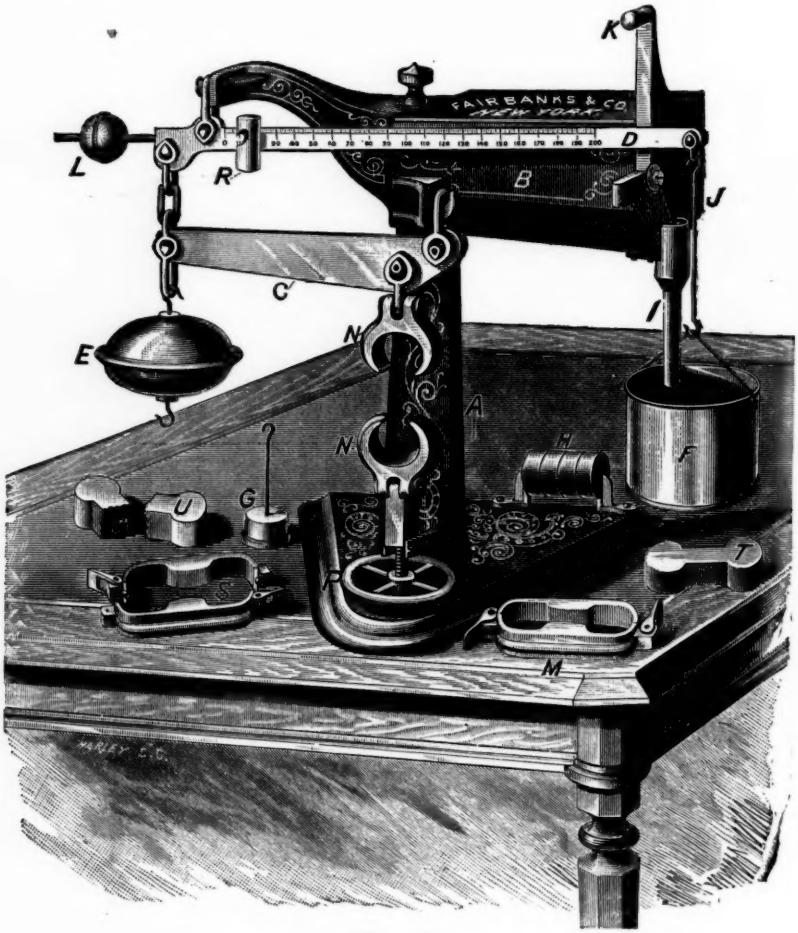


FIG. 25.

Perhaps next to iron and steel the material most frequently tested is cement, and in the illustration, Fig. 25, may be seen an automatic cement-testing machine. On the pedestal of the machine there stands a column A, supporting on the top a receiver B, to which is attached

is removed by opening the clamps and sliding the two side pieces away. After the briquette has attained the requisite age it is placed in the clamps NN, the hand wheel P is turned, drawing down the lower clamp and taking up the slack so as to raise the scale beam nearly or quite

to the top of the guard K. The reservoir B has previously been filled with shot, and by opening the valve J this shot is allowed to run from the tube I into the pail F. The weight of the shot flowing into the pail constantly increases the stress on the briquette between the clamps NN. As soon as this stress reaches the tenacity of the specimen in question, the briquette breaks, the beam D drops, and striking the valve J, shuts it up, cutting off the supply of shot. The pail with its shot is now detached from the end of the beam, and hung on the hook on the under side of the balance ball E. In the place of the pail is hung the counterpoise G, which exactly balances the weight of the pail, thus leaving only the amount of the shot contained therein to affect the scale. By sliding the poise R to and fro on the beam, or by adding on the counterpoise the weights H the amount of the shot required to break the cement may be readily and directly read from the beam. The beam is graduated to 200 lbs., and with the addition of four small weights a capacity of 1,000 lbs. is obtained.

The elasticity of car springs, and the amount which they will deflect under a given load has lately become a matter of much investigation, for the railway companies constantly increasing the loads in their cars and running at higher speeds, require springs which shall be correspondingly safer and more durable to support the heavy use to which they are subjected. Fig. 26 is an illustration of a machine designed to meet the wants of car spring manufacturers, so that every spring that is made may be, as fast as it is completed, tested, to ascertain whether or not it fulfills the specifications that it is intended to meet. A heavy cast iron framework supports a cross-head carrying an adjustable plunger. This plunger may be raised or lowered by means of a screw thread cut on it, so as to accommodate any length of spring from two inches to three feet. Directly beneath this plunger there is a

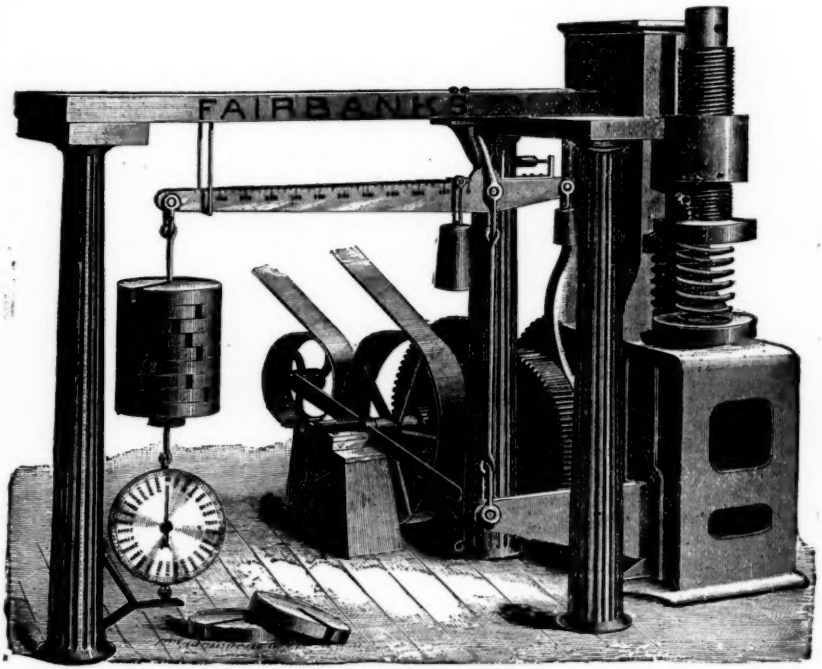


FIG. 26.

bed plate for supporting the spring, which forms a platform of scale. The other end of this lever stands on a lever directly inside of the casting that conveys the stress received by the spring to the scale beam, where, by means of a sliding poise and the large weights, its quantity may be correctly estimated. The gearing and the belt at the back of the machine form a means by which the plunger may be moved up and down. For example, supposing a railway company has issued specifications for 10,000 springs 6 in. in height, which shall carry 10,000 lbs. and deflect 7-8 of an inch under a load. The spring maker coils a spring which he supposes will answer the requirements, and placing it in a testing machine, so adjusts the cross-head that it shall depress the spring 7-8 of an inch. The machine is then started and the weights added to the scale beam until a balance is obtained at the given deflection of the specification. If the weight thus indicated corresponds with that of the specifications, the spring maker knows that the spring in question will answer the requirements, and he goes on to fill his contract. If, however, the spring be either too stiff or too weak, a corresponding change must be made in the article. Having completed the entire contract the machine is adjusted to give a deflection of 7-8 of an inch, and as fast as the springs are ready, each one is placed in the machine. A glance at the spring balance, attached at the underside of the weight counterpoise, will tell the exact number of pounds that the springs sustained with a deflection corresponding to that inserted in the specification. All the springs which fulfill the requisite of weight may be confidently shipped by the manufacturer, while those that for any reason fail, may be returned to the factory. With a machine of this kind the manufacturer has a guarantee that all of the entire product must be accepted by the railway company as fulfilling their specification.

THE USE OF THE TESTING MACHINE.

The investigation of the physical properties of materials used in construction may be analytically divided into four parts:

1st. The investigation of the qualities of a special material. For example, the engineer may wish to ascertain the tensile strength, ductility, and resilience of a special steel.

2d. The manufacturer may wish to ascertain variation in quality produced by a change in the chemical composition of the metal forming his output. For instance, in the processes of the open hearth or the Bessemer converter, changes in the relative proportion of carbon, manganese or phosphorus may produce the greatest changes in the physical properties of the steels produced.

3d. The manufacturer or the consumer may wish to ascertain the changes produced in material by means of the different mechanical processes to which it is subjected while being shaped into the proper forms required by different structures. A noteworthy example is the various methods by which eye-bar heads are formed.

4th. The actual strength, elasticity, or resistance of a member forming one of the parts of a bridge or other structure may be desired, either for information as to the best method of design, or from which the actual strength of the structure in question may be ascertained.

These various subdivisions will now be treated in their appropriate order.

CARE ON THE PART OF THE OPERATOR.

The manipulation of the testing machine in order to obtain therefrom the most accurate and reliable information, embracing all the facts and data to be obtained from an experiment, is an operation requiring on the part of the manipulator the greatest exercise of care and skill and watchfulness. In making experiments on ordinary test pieces of a few inches in length, the operator is constantly dealing with two sets of quantities: one the measurement of the size of the test piece, and the amount of deformation produced by the stress previous to the elastic limit—involves the use of the most minute quantities. An error in the measurement of the piece of a thousandth of an inch, a quantity far too small to be appreciated, except by the most delicate means of measurement, may induce errors in the result sufficiently large either to condemn an otherwise good material, or to accept one which is inferior. In the measurements of the

deformation of the piece under the action of the stress, the quantities are still smaller, reaching to a ten thousandth, or even to a millionth of an inch, so that it will be obvious that during this part of the operation the greatest care is necessary to observe most carefully and accurately all of the circumstances which take place. The measurement of the stresses exerted by the machine running to thousands or hundreds of thousands of pounds, requires an exercise of skill in quite a contrary direction. It is not only necessary to measure these stresses accurately, but at the same time the mind of the operator must have sufficient flexibility so as to turn rapidly and easily from the consideration of a very small quantity to one which is exceedingly large. The entire behavior of the piece under the operation of the stress is one which requires the minutest attention. The equal or the unequal yielding of one side or the other, the appearance of the piece as under the operation of the stress, the fibers are torn apart, the surface becoming blistered, seamy and uneven, appeal in the most eloquent manner to the eye of the skillful investigator. The amount of deformation produced by the stress, and the way in which it takes place, whether the piece uniformly yields under its stress, or whether the yielding takes place at one particular point, may convey most important information as to the character, qualities and suitability of the material under investigation.

Placing the test piece in the testing machine is an operation requiring most careful attention. It is necessary that the piece should be so arranged that its axis may coincide with the axis of stress produced by the testing machine. Even in the machines so designed as to furnish automatic means of centering it is still necessary for the operator to exert the most unceasing vigilance to see that the piece is correctly surrounded by the jaws of the machine, to insure that these jaws exert an equal bearing all over the surface, that it is properly set in line, and that the entire adjustment with reference to the various parts of the machine is done in the most careful and accurate manner. This adjustment of the test piece is especially essential in experiments upon metals which are crystalline, stiff, and unyielding, such as cast iron. Mr. Hodgenson states that the strength of a rectangular piece of cast iron so placed in the machine as to cause the stress to pass along the side of the piece is only one-third of the strength to resist a central stress. It is evident that this ratio would perhaps be given only by a few forms, subjected to tension in a peculiar manner, and that it probably would vary with every change in shape and position of the test piece. In machines not furnished with self-adjusting jaws the importance of this care in setting the test piece can hardly be too strongly insisted upon. It has frequently happened in the experience of the author that in making experiments upon plate iron the piece would be so placed in the testing machine as to be torn apart rather than fairly broken. In many experiments the bend of the test piece after rupture was markedly perceptible to the eye. The errors in such experiments may rise as high as twenty or thirty per cent. of the actual strength of the piece in question.

THE APPEARANCE OF THE FRACTURE.

After the completion of an experiment a careful examination of the test piece should be made, and the character and appearance of the fracture should be carefully noted as an index of the quality of the material. Good wrought iron, when broken by tension, should present a fracture resembling a bundle of fibers, having a dark gray color. There should be little or no appearance of lamination due to the piling and the rolling of the bar, and no appearance of any bright or crystalline spots. The fracture should be irregular, inclining towards cup shape. The general appearance of the fracture of good wrought iron is indicated in Fig. 27. In the poorer qualities of wrought iron it is very customary to find the bar interspersed with a greater or less number of bright, shining crystalline spots, similar in appearance to cast iron, excepting that the crystals have very much higher metallic luster. The relative proportion that the crystalline spots bear to the whole area of the piece may be taken as a very fair index of the quality of the material. Fig. 28 gives a representation of the fracture of the medium quality of cast iron, showing about one-third of its area to be com-

posed of these hard crystals. Such iron may stand a very high tensile stress, but it will show very little ductility. This crystalline structure of the bar may be due to one of two causes. Either the iron contains too large a proportion of phosphorus, and so is termed cold-short, being exceedingly brittle and unworkable when cold, or else during some processes of the manufacture the bar was overheated (burnt, as it is said in the blacksmith's shop) and allowed to cool with insufficient mechanical work. The cold-shortness attributable to too large a quantity of phosphorus is not indicated by any exterior appearance of the bar, whereas, if the iron has been burnt, the exterior is blistered and rough, showing to the practiced eye that there has been

FIG. 27.

overheating, by which the more fusible parts of the bar were actually melted.

The length or size of the fibers in the fracture is an exceedingly good index of the value of the material. Fractures which occur with short and apparently brittle fiber, rather fine in texture, generally indicate an iron which will when cold possess considerable ductility, yet probably has such a proportion of sulphur as to make it red-short and exceedingly troublesome to manipulate under the hammer. The results of irons of this kind may be fairly good in the testing machine, yet as far as is possible they should be avoided in construction on account of the difficulty of obtaining good results from shop manipulation.

During an experiment on wrought iron, as soon as the elastic limit is passed the surface of the specimen, if it has previously been machined, gradually loses its bright metallic luster and becomes rough and mottled under the distorting effect of the stress. In the finer and most superior irons this mottling of the surface is exceedingly delicate. The fibers of the piece seem to be rumpled and distorted by the stress so that the whole of the surface is thrown into an innnumerable number of minute ridges. In the coarser and less valuable irons this mottling of the surface is comparatively heavy and uneven and the want of homogeneity in the structure of the metal is clearly indicated by the uneven appearance of the test piece.

The fractured surfaces of steel bars

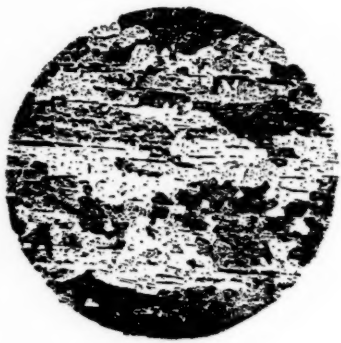


FIG. 28.

present a far greater variety than those of iron specimens, in fact there are at least a dozen or fifteen different appearances in steel indicating corresponding changes in the value of the metal. The appearance of the fracture of good constructural steel, having an elastic limit of from forty to fifty thousand pounds, and an ultimate of from seventy to eighty thousand is a light silky gray. One end of the test piece, broken by tension, being a slightly truncated cone that fits into a corresponding concavity in the other end. Steel fractures, excepting those of the poorer varieties, should rarely be termed fibers, for the texture of the metal is so exceedingly delicate as to be only comparable to the finest silk. Steels made by the Bessemer process generally pre-

sent a fracture which is darker and a little more corky in appearance than those made by the crucible or the open hearth methods. As the amount of carbon increases, the tensile strength and the elastic limit are increased, and at the same time the silky and conical appearance of the fracture gradually decrease, until, when the tensile strength rises to a hundred thousand pounds per square inch, the silky appearance is almost entirely lost, the fracture becomes square, showing little reduction in the bar and presenting a granular appearance made up of small brilliant crystals. The same appearance of the fracture may be produced by an excess of manganese. In such steels the tensile strength may not rise over sixty to eighty thousand pounds, yet the character of the fracture is by the presence of the manganese entirely changed from that of the fine delicate texture of the carbon steel to a coarse and granular appearance almost resembling that of cast iron.

The effects of phosphorus and sulphur are similar to those produced in iron. An excess of phosphorus renders the piece cold-short, reducing the amount of reduction and giving the fracture a more granular character.

The following classification of the characteristics of fractures is presented, hoping that it may in some measure aid towards uniformity in description.

STEEL FRACTURES.

FIRST, SQUARE.—Those in which the plane of the fracture is nearly at right angles to the axis of the test piece.

SECOND, IRREGULAR.—Those in which the plane of the fracture is inclined more or less to the axis of the test piece and in which the fracture presents one or more jagged points.

THIRD, CONCOIDAL.—In which one end of the test piece presents a cone matching in a corresponding conical socket in the other half.

AS TO MOLECULAR STRUCTURE.

FIRST, FINE CRYSTALLINE.—Fractures such as are presented by the rupture of the best quality of tool steels, in which the bar seem to be made up of an infinite number of exceedingly fine and iridescent crystals

SECOND, CRYSTALLINE.—Such fractures as are presented by the coarser qualities of tool steels, or by the higher grades of structural steel containing large amounts of carbon or phosphorus. Here the fracture is composed of larger crystals, losing to a great extent their iridescence and seeming to be grouped in radial lines around the center.

THIRD, SILKY.—Fractures which are manifested by the best varieties of structural steels in which the crystalline appearance is entirely lost, inasmuch as the crystals seem to be merged in long bundles of the finest and silkiest fibers.

FOURTH, GRANULAR.—Fractures of this kind are manifested by the inferior varieties of structural steels and seem to consist of a large number of granular crystals of considerable size, grouped in radial lines around a central axis.

FIFTH, COARSE GRANULAR.—Fractures of this class are only manifested by the inferior qualities of structural steels and are generally indicative of too high a per centage of manganese. In such cases the crystals are large and coarse, presenting many glimmering facets and having almost the appearance of the poorer qualities of wrought iron.

WROUGHT IRON fractures may be classified into square and irregular, as follows:

Square fractures are presented only by the inferior crystalline varieties that break sharply and shortly with but little or no reduction or elongation. As the quality of the iron becomes better, and the crystalline texture disappears, the fracture becomes more and more irregular, until, when a pure fibrous iron is attained, the fracture may occur at a plane inclined to the axis of the test piece at any angle, and presenting many jagged irregular points. The square fractures are always exceedingly crystalline, presenting a great number of large crystalline facets, having an exceedingly high metallic luster. The irregular fractures may be divided into long fibrous and short fibrous, depending simply upon the relative length of the fibrous structure.

Fractures of cast iron may be divided into, simply, fine crystalline and coarse crystalline, depending upon a relative size of the crystals. The coarser and less refined varieties of cast iron present, when broken, a fracture which is made up of a large number of dark grayish crystals, interspersed here and there with

traces of graphite. Upon remelting and refining the texture of the fracture, by each succeeding operation it becomes finer and finer, until, in the best refined iron, we have a fracture which is made up of exceedingly fine gray crystals, having but little metallic lustre and giving no evidence of graphite.

EFFECT OF THE SIZE OF THE SPECIMEN.

In making experiments upon the *qualities* of material the first point to be considered is the size and shape of the test piece and effect which this produces upon the results to be obtained from the testing machine. The variation of the form of the test piece considerably modifies the apparent tenacity of ductile material; consequently it is necessary to note the size and shape of the specimen to be tested, in order that the results obtained from the experiment may be correctly compared with those of previous or future results. When a piece of metal is subjected to tensile stress, and slowly drawn in two, it will yield at the weakest section first, and if that section is of considerably less area than the adjacent parts, or if the metal is crystalline and unyielding, it will break sharply and without appreciable stretch, the fractured surface having a granular appearance.

On the contrary, if the test piece has a uniform section of considerable extent, it will gradually stretch with a uniform reduction of section from end to end. Toward the ends where the piece is grasped by the machine this reduction of area is least perceptible. When the stress has obtained so great an intensity that the weakest section is strained beyond its elastic limit, the molecules of the metal commence to flow, and the piece gradually reduces in cross section, the particles immediately adjacent to the over-strained portion flowing towards the section of greatest stress. If, however, the shape of the specimen is so arranged as to prevent or to retard this flow of metallic molecules, the result is to largely increase the apparent tenacity of the material. This increase of tenacity due to the shape of the specimen has been suspected for many years past; but has only received a complete demonstration within a comparatively short space of time. The United States Board for making tests of iron and steel, made a series of experiments to ascertain the relative value of specimens having long or short uniform sections. The results of these tests are presented in Table I.

TABLE No. 1.

Prog. No.	Specimen L″	E. Lim lbs. □″	Ult St. lbs. □″	Elong. %	Reduc. %
1	5 00	26795	51989	30.	49.3
2	3.938	28194	52389	32.	50.0
3	4.500	28062	52495	30.	46.3
4	3.500	27268	53052	31.6	48.0
5	3.000		52984	33.0	48.0
6	2.472		52852	32.1	45.6
7	1.989		53169	32.9	45.0
8	1 500		52666	35.0	45.2
9	1.000		53169	35.4	43.5
10	.500		57318	41.6	36.6

Some experiments have given the following results from Bessemer steel specimens:

TABLE No. 2.

Grooved Specimen—

Highest	162974
Lowest	136490
Average	153677

Long Cylinder—

Highest	123166
Lowest	103255
Average	114460

In Fig. 29 may be seen the curves that have been obtained from a series of specimens of open hearth steel, made under the direction of the author in the following manner. From a bar of steel sixteen feet long there were cut twelve pieces. Each of these pieces was placed in a lathe, and one cut taken over it so as to render the piece perfectly straight and true. The series of steel pieces thus obtained, all cut from the same bar, were turned to lengths of uniform diameter, as seen in the column headed "Lengths" in the accompanying Table 3.

From these experiments it will be seen that the uniform length of the piece under examination exercises an exceedingly important part in the results obtained from the experiment.

In explanation of the cause of the difference in the flow, and the increased tenacity of the grooved specimen over one having for considerable length a uniform section, let it be supposed that a

TABLE No. 3.

Dimensions of Test Piece.			Stress Tension in lbs. per Square Inch.				Elongation in inches.	Reduction per cent.
Length in inches.	Diameter in inches.	Area in square inches.	Elastic Limit.	Maximum.	Ultimate.	On reduced Area.	Ultimate.	
Groove	1.000	.785	Elastic limit taken by fall of beam. Not perceptible before this point.	79286	78930	99614	Infinite.	20.77
.25	"	"		71138	70070	88530	100.00	20.85
.500	"	"		66446	64306	84983	60.00	24.33
.75	"	"		63036	61224	87114	46.66	29.72
1.00	"	"		62637	59707	87771	40.00	31.97
2.00	"	"		61671	57398	89463	30.00	35.84
3.00	"	"		61161	56122	94017	29.30	40.31
4.00	"	"	39642	61253	56226	94602	27.50	40.54
5.00	"	"	39667	61125	56226	94421	25.00	40.41
6.00	"	"	39515	60778	56250	94839	23.66	40.69
8.00	"	"	39235	60778	56226	94318	22.50	40.43
10.00	"	"	39158	60735	56250	94839	22.00	40.69

single chain of molecules of a ductile metal be subjected to tensile test, and let the length of this chain be ten inches. Under tension, each molecule would become separated from its neighbor, as far as their cohesive attraction would allow, on releasing the stress the molecules would resume their original places, no permanent extension having taken place. The greatest distance which one molecule could be separated from its neighbor would be an infinitely small quantity, and the total of the numerous extensions would probably not exceed one or two hundredths of an inch in the length of a ten-inch chain. This separation would represent the real limit of elasticity, which, if exceeded, would be followed by rupture. The stress causing this temporary extension would be the elastic limit, the tenacity and the tensile strength of the material. If the extension were carried beyond this, the chain of molecules would break, but each of the broken parts of the chain would show no indication of permanent set.

A test bar may be conceived to be made up of a number of these chains of molecules, each link having the peculiar property under high tension of leaving its own chain and taking up a position between the two nearest links of the next chain of molecules. These chains may thus become permanently elongated by the addition of new links, but they are at the same time reduced in number. The permanent elongation of the specimen is an indication that such a flow of molecules has occurred. The direction of this flow is from the exterior to the interior, as is proved by the reduction (always observed at the point of fracture) in a ductile material. Each chain in the aggregate may be supposed to be capable of bearing the same stress as if tested alone, or as if it had not become elongated by the addition of the neighboring molecules; but as the chains are fewer in number, the total stress carried by the whole, prior to rupture, is less than if the flow of molecules could have been prevented. For example, it is obvious that one hundred feet of ordinary iron chain, having a strength of one hundred pounds may, be divided into ten equal lengths of ten feet each, which by their combined tension might be made to support a load of one thousand pounds; but if the chain be divided into five parts, each twenty feet long, the united strength would only be five hundred pounds. In the case of a test piece, each molecule is not only a link of the chain in longitudinal direction, but it may also be regarded as a constituent link in all directions. In an ordinary ten-inch cylindrical specimen the end links of the transverse train of molecules come to the surface and are free. They therefore offer no resistance to flow in the transverse direction. In a grooved specimen the transverse molecular chains are not

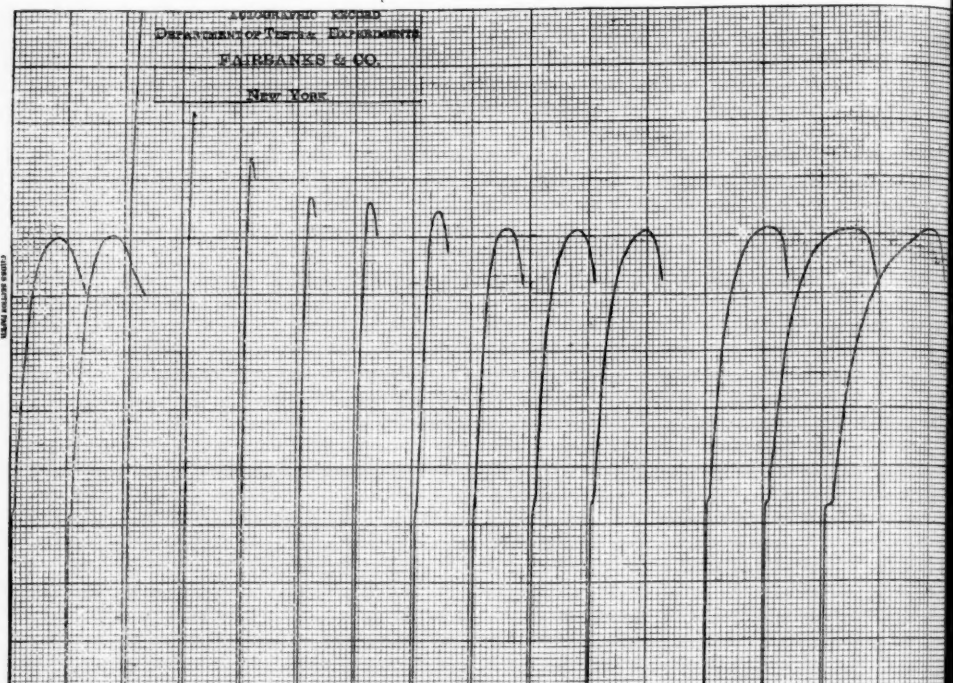


FIG. 29.

free, but are united to the ring of external metal. If this ring be of sufficient strength to resist the transverse stress, the reduction of the area of the piece would be almost, if not entirely, prevented, and consequently the tensile strength per square inch will approximate to the tenacity of this specimen—using the word tenacity to mean the strength calculated on the reduced section of the piece. Consequently, in a grooved specimen the additional strength thereby obtained may be regarded to be the sum of the tensile strength of the piece, plus the amount of force requisite to prevent the flow of metal and the ordinary reduction of area. In proof of this an examination of column of "tension per square inch of reduced area," in the above series of steel tests, will show that the ultimate tenacity

of the piece obtained by calculating the ultimate breaking strength as the reduced area of the piece, is nearly a constant quantity. To measure accurately the exact size of a broken test piece is a matter of considerable difficulty, and the variations in the foregoing column may probably be regarded to be due more to the inaccuracies in the measurements of the reduced area rather than to deviations from the above expressed law. Experiments of various grades of iron and steel have certainly indicated the probability that each particular grade of metal has its own ultimate of tenacity, so that if the tenacity of the piece be calculated upon the reduced area after fracture, a constant quantity would be obtained for each of the different grades of metal for every different length of test piece.

The importance of specifying in contracts for iron work, the exact size and shape of the pieces to be tested, has thus been rendered very obvious.

It is to be regretted that the size of test pieces has not been even more rigorously proscribed, and it is now suggested that engineers in framing iron and steel specifications, would do well to carefully indicate the exact size, shape and other circumstances governing the specimens to be tested.

THE EFFECT OF TIME OCCUPIED IN MAKING THE TEST.

It has previously been indicated that one of the first effects of stress on a ductile material is to cause the molecules to flow from the exterior towards the center. The question of the time occupied in making an experiment would seem to be an important one, and doubtless if the rapidity of the test is so great as to cause the stress to approximate towards a blow or a shock, the material has considerably less resisting power than it would have to a slowly and gradually applied stress. Yet the time occupied by most machines in making an experiment is so considerable that no approximation towards shock is ever reached.

In making a large number of steel tests for the East River Bridge, the question of the time to be occupied in making each experiment was gravely considered. The machine upon which most of the tests were made was worked by a hydraulic press actuated by hand power. In order to determine the effect of the time nine pieces of steel, one inch square and two feet long, were cut from the same rolled bar. These pieces were carefully measured and were placed in the testing machine without any preparation whatever. Three of the pieces were broken in ten minutes each, three were broken in six minutes each, and three were broken in twenty minutes each. The results obtained from these nine experiments only indicated a difference of some three or four hundred pounds, and no greater difference was observed between the test made in six minutes each, than those that were made in twenty minutes. It was inferred that on this particular grade of steel little or no difference could be attributed to the time occupied, provided it did not exceed the above maximum and minimum limits.

As a mean of a number of tests on wrought iron, Kircaldy found that with a suddenly applied stress an ultimate resistance of 46,500 pounds per square inch was obtained, while with a stress gradually applied the resistance rose to fifty-seven thousand two hundred pounds. Unfortunately the report from which the above is quoted does not give any information as to size or kind of specimens, or to the manner in which the suddenly applied stress was exercised. In making many experiments upon wrought iron and steel the author observed the following phenomena: If the testing machine be run comparatively slowly—so slowly that the motion of the cross head was a little slower than the natural flow of the metal—the test piece yielding gradually, giving a good elongation and reduction. By increasing the speed of the machine so as to quicken the motion of the cross head to such a speed as to a little more than keep up with the flow of the metal, a higher breaking stress was reached, accompanied with less elongation and reduction of area. By still further increasing the speed of the cross head a point would be reached where there was still less elongation and reduction and where the breaking strain was higher than in the first instance, and lower than that observed in the second instance. It would therefore seem that for each metal there is a particular rate of flow to which, if

the testing machine be carefully adjusted, the maximum resistance may be obtained. Yet, for the ordinary experiments upon a commercial product, or even for the purpose of comparing material with specifications, the time occupied in making the experiment may be considered as comparatively of slight moment, provided it be kept within the limits of from five to twenty minutes for each experiment.

Tests in tension require the observation of five points:

First, *the elastic limit.*
Second, *the maximum strength.*
Third, *the ultimate strength.*
Fourth, *the elongation.*
Fifth, *the reduction of area.*

In order of importance, as well as in order of occurrence, the elastic limit possesses by far the greatest interest to the engineer and the investigator. The elastic limit, as usually understood, is that point at which the metal, under the influence of the stress to which it is subjected, takes the first appreciable set. In the previous autographic diagrams it will be observed that the famous principle, known as "Hook's law," that the strain produced in the piece was proportional to the stress causing it is certainly true within the elastic limit. Here it will be observed that as fast as the stress on the test piece increases the pencil of the registering cylinder draws a straight line slightly inclined to the axis of Y at a constant tangent. This straight line continues until the elastic limit is reached, when, by a sudden breaking down of the molecular structure, a sharp point of inflection occurs and the curve turns rapidly towards the axis of X. Several methods have been used for the determination of the elastic limit. Formerly it was customary to place upon the test piece two center punch marks, occupying a distance from eight to ten inches. A pair of divides were carefully set so as to exactly coincide with the center punch marks. The piece was then subjected to stresses increasing by preconcerted increments; after the application of each succeeding increment or stress was removed, the dividers applied to the center punch mark to ascertain whether the piece had indicated any permanent set. It will at once be seen that this was a comparatively rough method of making so delicate a determination. In fact, strains smaller than one hundredth of an inch are scarcely to be observed by any such method. It will be readily seen that unless the test piece is very long, so as to present a very marked increase of length at the first permanent set, the elastic limit, as determined in this way, is almost certain to be several thousand pounds too high. An improvement on this method is obtained by the employment of micronometer screws, one or more of which are attached to the specimen, and the strain produced by the stress, together with the resulting set, if any, are recorded by measurements made with these screws. The objection to micronometer screws is two-fold. In the first place, the time occupied in making thirty or forty readings on a single test piece is so great as to make the operation of testing exceedingly tedious and wearisome; second, considerable personal equation is almost invariably introduced in ascertaining if the screw makes a fair contact.

To overcome this, electrical and frictional contacts have been devised, so as to insure that the pressure applied to the screw head, by the operator was a constant and uniform quantity. These methods have succeeded to a great degree. At the same time the micronometer screw, except in the hands of a very skilled operator, is liable to a considerable percentage of error. Again, the micronometer screw has to be placed at such a distance from the axis of the specimen that the slightest bending or twisting on the part of the piece introduces very grave errors into the results obtained.

Several forms of vernier gauges have been contrived, which may be applied directly to the specimen, and readings of extension and set obtained with them. One of the most compact and easily managed of these may be seen in Fig. 18.

Determinations of elastic limit, either by the micronometer or vernier gauge, generally give results which are five to ten per cent. lower than those obtained by the use of dividers and center punch marks. Prof. Thurston has shown that intermittent applications of stress to a test piece, especially at or near the elastic limit, tends to raise that quantity higher and higher. The author has therefore become accustomed to make determinations

of the elastic limit, simply by applying gradually increasing increments of stress to the bar, and reading the vernier gauge at the moment when the rising of the beam indicated that the requisite stress had been reached. So long as the increments of strain are proportional to the increments of stress, it is assumed that the strain on the piece is within the elastic limit. The moment, however, that the strain becomes disproportionate to the increment of stress, it is assumed that the limit has been passed.

Another method of very rapidly determining the limit in a testing machine worked by screws, is obtained by carefully regulating the speed of the machine with reference to the motion of the poises along the beam, and watching the motion of the beam itself. As an example in the machine used in the Department of Tests and Experiments, the multiplication between the end of the beam and the platform of the machine is 24,000. That is, in order for the platform to move downwards one inch, the end of the beam would have to move 24,000 inches. If the cross head of the testing machine be carefully driven by the screws, so as to apply the stress to the piece, no faster than the poise travels out along the beam, the moment that the elastic limit is reached and the piece under the application of the stress suddenly elongates, the beam will quickly drop to the bottom of its surrounding guard, and remain there stationary for a few moments, until the motion of the cross head catches up to the flow of the metal in the test-piece. Experiments on test pieces of various kinds have shown that this method of obtaining the elastic limit is fully as accurate as that by means of either the micronometer screw or the vernier gauge. It, however, does not give the amount of elongation that the test piece has undergone, and so is comparatively useless in those investigations requiring that elongation, for the purpose of calculating the modulus of elasticity. The observations on the elastic limit are by far the most important as furnishing data for the guidance of the engineer and the architect in construction, for it is now a well-established fact that if material is never strained to a point approaching the elastic limit, it is safe for an indefinite life; consequently, the chief point to be observed in making experiments on materials is the careful determination of the elastic limit and the modulus of elasticity.

THE MAXIMUM STRENGTH.

After passing the elastic limit, the curve on the cross-section paper proceeds forwards gradually, inclining more and more towards the axis of X, and assuming a general parabolic form. After proceeding in this way for some time, the maximum strength of the material is reached. At this point the rapidity of the application of the stress is equaled by the flow of the molecules in the test piece. For a few moments the curve becomes approximately parallel to the axis of X, and the entire work done by the testing machine is consumed in moving the molecules from the outside of the test bar towards its interior. In a few moments, however, this rapid movement of the molecules is attended by contraction in the size of the bar. As the result of this contraction there are a less number of molecules in the cross section to resist the force applied to the test piece, consequently the stress begins to decrease, the flow of the molecules becomes more and more rapid, until at last the rupture of the piece is obtained. In previous experiments very little account has been made of the difference between the maximum and the ultimate stresses to which the test piece in tension is subjected. In fact, excepting in those machines in which the weighing is done automatically, this difference is hardly perceptible, and even when it is perceptible, it is difficult by hand to remove the weights from the testing machine with sufficient precision to catch accurately the ultimate stress at which the piece is ruptured. By means, however, of the autographic testing machine the entire behavior of the test piece may be readily observed. Generally the ultimate and the maximum are regarded as almost one and the same quantity. In steel and wrought iron it will be seen from the foregoing diagrams that they are exceedingly different. Whereas, in experiments made on brittle and hard materials, having little or no ductility, these quantities are so nearly the same as not to require any special mention.

LONG COLUMNS—A NEW DEMONSTRATION OF PROFESSOR ROBINSON'S FORMULÆ.*

By W. S. WESTON.

Contributed to Van Nostrand's Engineering Magazine.

Having lately published tables computed according to Professor Robinson's formulæ, the author feels justified in presenting in this article a new demonstration of formulæ which he believes to be perfectly rational.

Professor Robinson's column formulæ differ very essentially from other rational formulæ that have been presented hitherto in the introduction of a new relation which limits and gives a maximum value for the deflection of the column. A little thought will show that such a limit does exist. If a column is loaded it deflects; increasing the load increases the deflection. Is there not a limit to this deflection at which, if the load be further increased the column will fail? Professor Robinson called attention to this fact; determined the value of the limit, and then with obvious propriety used it in producing new formulæ for columns.

It has been objected by Professor Wm. H. Burr, in the November number, 1883, of this Magazine, that Robinson's and all other rational formulæ are vitiated by the assumptions of the common theory of flexure; and also that Robinson's are only Euler's formulæ with a mixture of errors. Hence, before proceeding to demonstrate and apply the limiting value of the maximum ordinate y, (the nomenclature in this article being the same as that used by Professor Robinson, see *Van Nostrand's Magazine*, vol. 26, page 490), it will be well to consider the effect of the assumptions of the common theory on the reliability of the formulæ, and to justify the use of Euler's formulæ.

The assumptions of the common theory of flexure are four in number: *First*, the co-efficient of elasticity is assumed to be constant; *second*, a plane section of a beam or column before flexure is assumed a plane after flexure; *third*, the resistance to compression is assumed equal to the resistance to tension; *fourth*, the reciprocal of the radius of curvature is assumed equal to the second differential coefficient. Other assumptions, differing in name, will be found on analysis to be but modified forms of these four. That for incipient flexure these assumptions represent actual conditions must be admitted by all. The question is, at what limit of flexure will they cease to be justifiable? It is contended that this limit is never reached in a safely loaded column. The following are the reasons:

By experiment we know the coefficient of elasticity is practically constant within a limit always recognized in loading columns. The error of the third assumption can not affect the formulæ, because the neutral axis is generally without the column, and, as will be shown, may never approach nearer the center than the radius of gyration. The second assumption is justified by our knowledge of the infinitesimal nature of the change in a section of a metallic column when safely loaded. The error of the fourth assumption can be computed and is known to be very small for small changes of curvature. This will be true for stable columns. For, a short column fails by crushing, mainly, while a long column fails by bending when, as will be shown, its deflection exceeds the radius of gyration; and hence the change of curvature in either case will be small.

One other assumption is made in the development of the new formulæ. A load on a column produces compression by virtue of its bending effect and also by virtue of its being transmitted through the column. It is assumed that the total resistance to compression is equal to the sum of the resistance to compression due to bending and the resistance to direct compression, these actions being conceived to take place independent of each other. There is no reason for questioning this assumption for strains within the elastic limit.

* This article was sent in for publication before the appearance of Professor Robinson's answer to Professor Burr in the April number of this Magazine.

With this brief discussion of the assumptions we are now ready to establish the old as well as the new relations employed in developing the new formulæ. For the sake of simplicity only columns with round ends will be considered.

Question has been raised as to the propriety of using Euler's formulæ

$$\frac{T}{\varepsilon I_1}=\frac{\pi^2}{l^2} \quad . \quad . \quad (1)$$

If this does not give the strength of a column, how can it be used to produce an expression which will?

Equation (1) is deduced from the general equation of moments,

$$\frac{\varepsilon I_1}{\rho}=Ty_1 \quad . \quad . \quad . \quad (2)$$

and results from the fact that the elastic curve of a stable column is a sinusoid. The elastic curve is that curve which the axis of the column takes when the material is nowhere strained beyond the elastic limit. These equations (1) and (2) are thoroughly theoretical and do not recognize the fact that the material has what may be called perfect elasticity only within very narrow limits. It is in the use of the quantity represented by the symbol ε that our trouble is to be found. In these equations, ε enters as an absolute quantity, whereas, it can not be and never was intended to be used otherwise than as a coefficient to denote the elastic nature of material within a very close limit. The coefficient of elasticity does appear in the new formulæ, but, as will be shown, in its true office of a coefficient. The quantity T in (1) and (2) may be looked upon as a load which, if the material has unlimited perfect elasticity, would give the column its elastic curve. It may also be concluded that so long as the curve of a column is the elastic curve, so long will (1) and (2) be true equations. But there is little in equation (1) to show how much a column will bear before its curve will cease to be an elastic curve. We know that not only the length of the column, but also the elastic limit, and a limit to the deflection, must be considered in determining the load.

It is plain then that neither (1) nor (2) can be used for a column formula. A legitimate and very proper use, however, is made of these expressions. It is desired that the curve of a loaded column shall always be the elastic curve. The above equations imply this condition. If then we can obtain from them a relation independent of ε, we shall be justified in using it in the solution of a new formula. Combining (1) and (2) gives

$$\frac{1}{\rho}=y_1\frac{\pi^2}{l^2} \quad . \quad . \quad . \quad . \quad (3)$$

Equation (3) is an equation of condition. When it is true, perfect elasticity exists, as shown above. When it is not true, perfect elasticity has been exceeded, and the column will be unsafe. Therefore, in view of these conclusions, and the fact that the elastic curve should be recognized, we are justified in the use of Euler's formula.

It will be admitted that the resistance to compression within the elastic limit should enter the formula. But, as noted in the fifth assumption, the compression consists of two parts. Let t equal the total resistance to compression; t_1, the resistance to compression due to bending, and t_2, the resistance to direct compression. Then,

$$t=t_1+t_2=Ty_1\frac{d_1}{I_1}+\frac{T}{K} \quad . \quad . \quad (4)$$

The value t_1 is found from the equation of moments,

$$Ty_1=\frac{t_1}{d_1}I_1 \quad . \quad . \quad . \quad (5)$$

It is now necessary to mark carefully the difference between equation (5) and equation (2). They are both true so long as the column preserves its elastic curve; that is, so long as the material is strained within its elastic limit. One equation recognizes this limit, while the other does not. Equation (2) implies, in the use of e, the existence of unlimited perfect elasticity; while (5), by the use of t_1, recognizes this quality only within limits well defined by experiment. We may say then with truth that T, as given by (5), is a much more reliable and a safer quantity than T, as given by equation (2).

To the relations expressed by equations (3) and (4), Professor Robinson has added a third. The limit and maximum value for the maximum ordinate y, may be proved in the following manner:

Take the moment equations,

$$\frac{\varepsilon I_1}{\rho}=Ty_1 \quad . \quad . \quad . \quad (6)$$

and

$$\frac{\varepsilon I}{\rho}=\frac{\varepsilon I_1}{\rho}+\frac{\varepsilon}{\rho}K\cdot\overline{BD}^2=T(y_1+BD). \quad (7)$$

in which BD is the distance from the center of gravity of the cross-section to the real neutral axis. Subtracting (6) from (7) gives

$$\frac{\varepsilon}{\rho}K\cdot\overline{BD}^2=T\cdot BD$$

Then dividing by BD and multiplying by y_1 we get

$$\frac{\varepsilon}{\rho}K\cdot BD\cdot y_1=Ty_1$$

But since in (6) $I_1=Kk^2$, k being the principal radius of gyration, we will have

$$\frac{\varepsilon}{\rho}K\cdot BD\cdot y_1=\frac{\varepsilon}{\rho}Kk^2$$

Therefore,

$$BD.y_1=k^2 \quad . \quad . \quad . \quad (8)$$

This is a true relation and is independent of the quantity T.

Equation (8) shows that the ordinate of deflection varies inversely as the distance of the neutral axis from the center of gravity of the section. For

$BD>k$, $y_1<k$; $BD=k$, $y_1=k$: and

$BD<k$, $y_1>k$.

If the ordinate has a limit it must be sought by discussing the equation of moments.

In (7) the moment of internal resistance is placed equal to the moment of the external force. Note now the effect of varying the value of BD in this equation. First, taking $BD=k=y_1$, and substituting in (7), we find a true equality verified by (6). Second, taking $BD>k$, and hence $y_1<k$, we find that the first member of (7) becomes the greater, or that the internal moment is greater than the external. Third, taking $BD<k$, $y>k$, we find the first member the less, or the internal moment less than the external. But the moment of internal resistance should never be less than the moment of external force. Hence, the conclusion drawn from this discussion is this: BD may never be less and y_1 never more than k, and at the limit $BD=k=y_1$. In other words, the neutral axis of a safely loaded column may never approach nearer the center, nor the deflection be greater, than the principal radius of gyration of the cross section.

Equation (8) is an important relation, and may be deduced independent of the equations of moments. The resistance in the section of a column is undoubtedly analogous to the resistance of free bodies. When a free body is acted upon by an external force, it begins, by virtue of its resistance, to rotate about an axis which is called the axis of spontaneous rotation, or more properly the axis of *incipient* rotation. So also, when the resistance in the section of a column is brought into effect by an external force, the section will *begin* to rotate about an axis which passes through the neutral axis of the column. In both cases the axis of rotation is an axis of zero strain. Now, by a well-known principle of analytical mechanics, the distance from the center of gravity of the body to the axis of incipient rotation, multiplied by the distance from the center of gravity to the action line of the external force, is equal to the square of the radius of gyration. But BD is the distance from the center of gravity of the cross-section to the axis of incipient rotation, that is, to the neutral axis, and y_1 is the distance to the action line of the load. Therefore, $BD.y_1=k^2$.

Professor Robinson adopts at once the limit $BD=y_1$. It is proposed in this article, however, to wave for the present the choice of this limit, and to use in the further demonstration of the new formula the expression $BD=ny_1$, where n may be any value greater than unity. The effect of giving n its limit value *one* may be more thoroughly discussed when it appears in the column formula itself.

Let ad represent the shortening for a unit's length of the fibers which will fail first, and $(BD+d_1)$, the distance of these fibers from the neutral axis (see Fig. 21, *Van Nostrand's Magazine*, vol. 26, page 490). Then we shall have the proportion

$$ad:(BD+d_1)::1:\rho.$$

From this, substituting for BD its value ny_1,

$$ad=\frac{ny_1+d_1}{\rho}$$

Now, remembering that the shortening ad is to be produced by stresses always within the elastic limit, we shall have for the resistance to this shortening.

$$t=\varepsilon . ad$$

Whence

$$ad=\frac{t}{\varepsilon}=\frac{ny_1+d_1}{\rho} \quad . \quad . \quad (9)$$

Since t has been determined by experience it is plain that the quantity ε enters this equation as a coefficient qualifying the value of the second member.

Combining (3) and (9) and solving for y_1 we get

$$y_1=\frac{d_1}{2n}\left(\sqrt{1+\frac{4ntl^2}{\varepsilon d_1^2\pi^2}}-1\right) \quad . \quad (10)$$

Substituting this value of y_1 in equation (4), and transforming we obtain the formula for round end columns

$$T=\frac{tK}{1+\frac{Kd_1^2}{2nI_1}\left(\sqrt{1+\frac{4ntl^2}{\varepsilon d_1^2\pi^2}}-1\right)} \quad (11)$$

Now it will be seen that for $n=1$, the formula gives a value less than it does for n equal to any quantity greater than one. Hence the choice of the limit BD$=y_1$ will not affect the safety of the formula. This method introduces an approximation which, however, is quite small because of the position in the formula in which n is found. Omitting n, we get the formula as given by Professor Robinson.

In the same manner may be produced the formula for columns with ends fixed, and ends fixed and round.

It now becomes our duty to consider an objection which, so far as the writer knows, has not been made hitherto; but which is really the only well-founded objection to the new formulæ. It will be noticed that, although a maximum limit for y_1 has been determined by choosing BD$=k=y_1$ the minimum limit of BD, and, therefore, $n=1$; yet y_1, as given in equation (10,) is still an unlimited variable dependent on the length of the column. There is then an objection to the unqualified use of (10), inasmuch as it does not recognize the fact that the deflection of the column may never exceed the radius of gyration.

It is obvious that the deflection before incipient failure is quite small for short columns, and also that this deflection will increase as the length until it reaches its maximum limit. If a column fails with its deflection less than the limit, we know that the failure was caused directly by a failure of the material; but in case the deflection is equal to or exceeds the limit before the material itself fails, we know the column was in unstable equilibrium as soon as its deflection passed the limit, and that therefore the failure was caused, primarily, by excessive deflection. Now the new formulæ are to be applied only to columns which will fail directly by a failure of the material. Hence, equation (10) may be used only so long as it will give a value of y_1 less than k. Substituting for y_1 and n their limit values k and **1**, and solving for l, we shall obtain the length of column (in terms of k and d_1) which marks the limit of application of the formulæ. Fortunately, by giving to t the value of a safe working stress, the length as thus found is beyond the range of ordinary practice. The lowest limit of length belongs to solid round columns with round ends, and with $t=$ 10,000, and $\varepsilon=27{,}000{,}000$ is equal to $283k$. If we recognize, then, the limit of length for which y_1 will have a value less than k, there can be no objection to the use of equation (10). Although the formulæ are not completely universal as they were supposed to be, yet they are still sufficiently so for all kinds of columns having practical lengths.

Finally, from the foregoing demonstration and conditions there implied, we are prepared to draw the following conclusions:

First, the new formulæ are perfectly rational. The demonstration itself is the proof of this conclusion.

Second, the new formulæ can not, with justice, be compared with experimental results obtained in the usual method of breaking columns. It is a question whether practice would really desire the new formulæ to agree with the results of experiments for ultimate strength. It will be remembered that, in the demonstration, it was conditioned that the material should never be strained beyond the elastic limit. Under this condition the failure of a column is considered to begin at the moment when the elastic limit has been exceeded. This is exactly the condition which should be recognized in practice. With this definition of incipient failure, the new formulæ will give the strength of a column only when t is given a value within the elastic

limit. In the usual method of testing the strength of columns the material is strained to its ultimate resistance and considerably beyond its elastic limit. But the conditions near the point of crushing are quite different from those within the elastic limit, and from those belonging to an elastic column.

The irregular agreement of experimental and computed results, when t is given a value approximately equal to the ultimate resistance, does not prove or disprove the new formulæ, but, on the other hand, rather weakens the confidence in the usefulness of the experimental results for ultimate strength. It is believed that ultimate resistance is not so important a factor in practice as the elastic limit. The ultimate resistance should be looked upon only as a margin for safety in case of strain beyond the limit. If comparisons between the formulæ and the results for ultimate strength are made, t should be given a value greater than the experimental result, since the ultimate resistance of the material is necessarily greater than the ultimate strength of the column.

With the doubt as to whether the ultimate strength of experiment can be used with confidence, comes the doubt as to the reliability of empirical formulæ. This is but another proof of the superiority of rational to empirical formulæ.

Experiments may be devised to determine the greatest load which a column will bear and remain stable, and still retain its perfect elasticity. Such a method of testing is exemplified by that used in the testing of finished structures. A bridge is tested not by a breaking load but by a load which, according to the calculations, should not strain the material beyond the limit; and the return of the bridge to its former condition after the removal of the load is considered an evidence of its strength.

To sum up, it may be said of the new formulæ: they are perfectly rational; they apply only where the material is strained within the elastic limit, and therefore give the strength of an *elastic column*: and they are applicable to all kinds of columns and for all lengths within the range of ordinary practice, when t is given the value of a safe working stress—a value considerably within the elastic limit.

NOTES ON GEOMETRY, WITH ESPECIAL REFERENCE TO THE METHODS EMPLOYED.

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II.

GEOMETRY is justly considered one of the most exact of sciences, since the axioms and definitions that form its bases are regarded as such that every one is willing to admit, whilst the propositions are just deductions from the true assumptions.

It is evident, however, that if any of the hypotheses can be simplified, there will be a real gain in the perfecting of the science, which should be founded on the fewest number of things regarded as self-evident, especially if there should be the slightest suspicion that any of them may be contradictory. Again as to the methods of reasoning employed in deducing the various propositions from the given data, there is evidently choice, though modern authors have confined themselves so strictly to the deductive method, that the inductive or ancient analysis has scarcely received a passing notice from them. As the latter method is of such great value in scientific discovery, its complete exposition as a perfect method of reasoning, when rightly understood, is very important, and is well worthy of the most attentive consideration.

In developing the subjects under consideration, a brief exposition of general methods, applicable to all sciences of reasoning, is first given as naturally preceding the critical notes and applications to geometry.

Whatever of value is found in the following pages is due mainly to Duhamel, whose "Méthodes dans les Sciences de Raisonnement" has been freely used in the preparation; and it is hoped that none of the acute, searching thoroughness, so characteristic of that author, has been lost in the transfer.

Methods Common to all Sciences of Reasoning.

1. Necessary truths exist by themselves; reasoning and methods are only means that man employs to discover them or to recognize them, and their object is to produce in him the knowledge and *certainty* of the truths.

This state of certainty is caused in man by the clear conviction of the truth; in other words, as Descartes long ago observed, by the *evidence* or proof of it.

Truths whose obviousness occurs to all minds are used as the starting point, and the *methods* serve to discover other truths which can then be admitted with the same certainty as the first.

2. The ordinary form of the *syllogism* is a form of reasoning in which the characteristics belonging to a group are simply reaffirmed with respect to the individuals of that group, which is so simple that it scarcely needs giving a name to. It does not help to discover anything, for we must know or ascertain the qualities of the individual before we can affirm them for the group, and therefore it will not be included in the general exposition of methods.

3. The errors that we commit in reasoning proceed less often from a vicious deduction than from the inexactness of the propositions admitted, which are often, not those called *axioms*, which every one would admit from experience to be true, but rather plausible hypotheses, not so evident, which may be true for special cases, but are not so in all their generality, which, however, is often assumed as evident.

The only way to escape error in this direction is to examine carefully and perhaps tediously, every case that the statement involves, and verify it in each instance, or state the exceptions where the statement is not general, before proceeding with the investigation.

4. The operation of deducing a result from known results is called *deduction;* it corresponds to *synthesis*, and is the method generally used in communicating truths, that have been previously ascertained, to others.

5. The method of *induction* is the operation by which we refer the knowledge of a thing to that of other things of which it will be a consequence.

This corresponds to the *ancient analysis*, and is generally the march of discovery. It will therefore be fully developed as we proceed.

6. Propositions that cannot be true at the same time are said to be *incompatible*. Thus, to say that x is greater than y is incompatible with the statement that y is greater than x; but both statements can be false at the same time, which would happen if x were equal to y.

But if each proposition is the negative of the other, or *contradictory* to it, if one is true, the other, which denies it, will be false; and, if one is false, the other, which only denies it, will be true. We must be careful, though, not to omit any of the cases of the contradictory proposition, otherwise we should have a proposition that was simply incompatible with the first. Thus, if we assert that $x>y$, the contradictory proposition would assert that x was not greater than y, which includes two cases, viz., $x<y$, and $x=y$. If we simply said that $x<y$, the two propositions would be incompatible, but not necessarily contradictory, for both would be false if x equals y, so that we could not, if we recognized the falsity of one, thereby infer the truth of the other.

7. It must be carefully borne in mind, also, that truth may sometimes be deduced from falsehood, so that it does not follow because a deduction is true that the propositions from which it is deduced are true. They may be, one or more, false.

Thus, Aristotle gives the following odd illustrations, in the first of which the two premises are false, and the conclusion true:

Man is stone;
All stone is animal;
Then mankind is animal.

In the following, one premise only is false and the conclusion true:

All horses are animal;
Man is not animal;
Therefore, a man is not a horse.

Many illustrations could be given, but it suffices to remark that we may reach a true result from a false principle, either because the latter is a mixture of true and false, and we employ only the true, or that the errors made in the deductions compensate each other; thus it is proved that if we reach a true result, it does not follow that the relations admitted in deducing it are true.

8. It is equally important to observe, that if in going from certain relations we deduce, by just reasoning, a false conclusion, that *the first relations are not all true.*

For if they were we could only deduce true conclusions.

9. The science of a thing is the whole of the laws of that thing, so that the science of reasoning involves the laws of reasoning.

10. A *theorem* is a proposition to be demonstrated; a *problem*, something to be done, satisfying given conditions.

In a problem we indicate the result we wish to obtain and demand the means of attaining it. A theorem is true or false, a problem possible or impossible.

11. *Analysis.* The "*ancient analysis*," which alone we shall consider in this paper, does not seem to be well known by modern logicians. We find the first traces of it in the elements of Euclid, though Pappus of Alexandria refers the invention to Plato.

The method can be applied both to theorems and problems, which applications will be considered separately.

12. *Analytical methods for the demonstration of theorems.*

When we have to find the demonstration of a given proposition we shall seek first if it can be deduced as a necessary consequence of admitted propositions, in which case it should be admitted; it will thus be demonstrated.

If we cannot perceive from what known propositions it can *be deduced*, we shall seek from what proposition not admitted it can be, and then the question will be to demonstrate the truth of the last. If this *can be deduced* from admitted propositions it will be true, and consequently the proposed will be; otherwise we shall seek from what proposition not admitted it *can be deduced*, and we then have to prove the truth of the last, if possible.

We shall continue thus until we reach a proposition that may be recognized as true, and then the truth of the proposed will be demonstrated.

We see, therefore, that this method that we call analysis consists in establishing a chain of propositions, commencing with the one we wish to demonstrate and ending with a known proposition, so that in going from the first, each may be a necessary consequence of that which follows; whence it follows that the first is a consequence of the last, and therefore as true as it is.

13. *Definition.* When two propositions are so related that either one can be deduced from the other as a necessary consequence they are said to be *reciprocal.*

14. Now, if any two successive propositions, found after the general method above are reciprocal, we can consider the second as deduced from the first, and *if this reciprocity holds from the first to the last*, we can say that the analytical method consists in establishing a series of propositions of which the first is the one to be demonstrated, and such that the second is deduced from the first, the third from the second, and so on to the last proposition which is recognized as true.

15. It is by no means necessary that all the successive propositions be reciprocal; only if some are not so then the general method of art. 12 must be applied to such propositions.

If we employ the second manner of proceeding (art. 14), which is often easiest (as it is generally simpler to deduce a consequence from a proposition than to find another of which this will be a consequence), and do not ascertain that the successive propositions are reciprocal, if for only two, say; the second of the two can be true without the first being true, *since the truth can sometimes be deduced from error* (art. 7); so that we can not say that the first proposition is demonstrated.

Duhamel is the first author who has insisted on this reciprocity test, and it is well to call especial attention to it, for its neglect may lead to grave errors. Of course, where it does not obtain, the general method of art. 12 must be used.

16. *The analytical method for the solution of problems.*

The object of a problem is to determine one or more things of given kinds, so as to satisfy given conditions. If we

can refer the problem, then, to another whose conditions satisfy the first, so that the knowledge of the second comprehends at least some solutions of the first; if the new problem is easier to solve than the first, the question will be advanced.

If the second cannot be resolved, refer it to another as the first was referred to it, and so on until we reach a problem we can resolve. Its solution will furnish one or more solutions of the proposed.

17. Now, in this general method the successive problems have been so chosen that the conditions of any problem satisfy those of the preceding. Now, if the conditions of any one satisfy as well those of the succeeding one, the two conditions are said to be *reciprocal.*

If this does not obtain, the general method may not find all the solutions, so that there may be *lost* solutions in this case; for calling any problem (1), and the succeeding one, whose conditions satisfy those of the former (2), it is plain that a solution of (2) will give some of the solutions of (1) for the same conditions or requirements that satisfy (2) satisfy one; but some solutions of (1) may not be solutions of (2), for, by assumption, all the conditions of (1) do not necessarily satisfy (2), so that there will be more solutions of (1) than we should find by solving (2). But if, on the contrary, the conditions of the second problem and those of the first are reciprocal when the first is satisfied the second is also, therefore all the solutions of the first problem will be solutions of the second, and *vice-versa;* so that if we find all those of the second none will be lost.

Reasoning in the same way for the others, we can state generally the following proposition:

If in the problems that we substitute for the preceding, in going from the proposed, the conditions of any two consecutive ones whatsoever are reciprocal, the solutions of the first involve those of the last, and of any one whatsoever of the others, and reciprocally.

In the above reasoning we have not required that the things demanded be the object of research in each problem; but if they are different, we call the conditions of the two problems reciprocal, when knowing the things satisfying the imposed relations in either one of the problems, the corresponding things of the other satisfy the conditions imposed upon them in this other; in other words, when the complete solution of either problem entails the complete solution of the other.

18. It may conduce to clearness to mention the successive steps in the analytical solution of a problem given by Duhamel in his second book, which is too long to reproduce entire.

PROBLEM—*To draw a circle tangent to three given circles.*

It is easily shown that the solution of this problem involves that of this problem, *to describe a circle which passes through a given point and is tangent to two given circles* and *vice versa.*

The two problems thus have reciprocal conditions, for the things satisfying the conditions of the one require as a necessary consequence that the corresponding things satisfy the conditions of the other.

We have thus referred the given problem to another whose solution embraces that of the first.

Similarly we refer the second problem to a third;

To describe a circle which passes through two given points and is tangent to a given circle,

whose solution involves that of the second, the conditions being reciprocal; when again, we refer the last to the problem,

To pass a circle through three points,

and with reciprocity.

It is to be understood that the constructions effected in referring the first to the succeeding problems have all been made, so that when the centre of the the circle corresponding to the last problem is found (which we know how to do), this is none other than the centre of the required circle, which is thus easily drawn tangent to the three circles, and the original problem is solved.

19. Let us next take the case, where the conditions of the new problem substituted for the first, are simply consequences of those of the first, but not reciprocal to them.

It follows that any solution of the first involves the solution of the second, since all that satisfies the conditions of the first, necessarily satisfies the conditions of the second; but *all* the solutions of the second are not necessarily solutions of the first, since the hypothesis is that the con-

ditions are not reciprocal, and it will therefore be possible to satisfy the conditions of the second without satisfying those of the first. The second problem in this case has, then, *all the solutions* of the first, but it may contain *strange* solutions in addition.

20. REMARK.—The writer is constrained to think that the reasoning of Duhamel in the preceding article is obscure and possibly requires elucidation. First, it seems to be tacitly assumed that the second problem substituted for the first is not simply *a particular case* of the first, for then the solution of the second might not include *all* the cases of the first, but that the conditions of the second are just as comprehensive as those of the first and include every case of the first, so that the solution of the second gives every solution of the first. But then it may be asked, why should it contain more, if it is *simply* a consequence of the first? How can a consequence, if it is simply a consequence and nothing more, include more than the original proposition?

The truth is, that in the examples illustrative of "strange solutions," it will be found that the so-called consequence, is a consequence only for certain values of the quantities entering it; whereas, in solving it, we tacitly assume it to be true for all values of its quantities, so that we find strange solutions to the original problem.

In fact the assumption, in art. 19, that the old and new conditions are not reciprocal, implies that the new conditions are more comprehensive than the first, and that only part of them are direct consequences of the first; for if all were, there would necessarily be reciprocity. For these reasons, I prefer to put the deductions of article 19 in the following form:

When the conditions of the old and new problems are such, that *all that satisfies the first satisfies the second, but not reciprocally*; then the complete solution of the second problem will certainly give all the solutions of the first, with perhaps *strange solutions*, if the conditions of the second are more general than those of the first and not all strictly consequences of the first. The solution of the following problem will elucidate what has been said:

21. PROBLEM.—*Knowing the hypothenuse of a right triangle, the sum of the other two sides and the altitude, to compute the last three quantities.* Call a this sum, b the hypothenuse regarded as the base; x and y the two sides, and z the altitude. The known properties of the triangle and the relation imposed in the enunciation give the three following equations:

$$x+y+z=a \quad . \quad . \quad . \quad . \quad (1)$$

$$xy=bz \quad . \quad . \quad . \quad (2)$$

$$x^2+y^2=b^2 \quad . \quad . \quad . \quad . \quad (3)$$

Thus we refer the geometrical problem to one of numbers. To solve the latter, transpose the z in eq. (1), and squaring, we have,

$$x^2+2xy+y^2=(a-z)^2 \quad . \quad . \quad . \quad (4)$$

subtracting eq. (3) from this, and double eq. (2) from this result we obtain, on reducing,

$$z^2-2(a+b)z+a^2-b^2=o \quad . \quad (5)$$

whence,

$$z=a+b\pm\sqrt{2ab+2b^2} \quad . \quad . \quad (6)$$

But it would evidently be wrong to take the solution that corresponds to the + sign of the radical, for z, the altitude of the triangle cannot be greater than $a+b$; whence substituting the other value in (1) and (2), we find the values of x and y by elimination between the first three equations.

But, we can ask, why have we found two values for z when one only applies, the other being "strange" to the solution. Duhamel says, "now we have truly demonstrated, that eqs. (1), (2) and (3) were *consequences* of the geometrical conditions, but not the reciprocity, whence the strange solutions." The truth is (1), (2) and (3) are not *simply* consequences of the geometrical conditions, but enclose in themselves other solutions, *for if x and y are both written with the minus sign*, we shall obtain the same value of z, eq. (6) just found. This is plain, for eq. (4) can equally be written $(x+y)^2=(a-z)^2$, or $(x+y)^2=(z-a)^2$ and the results of the calculation will be the same. Therefore, eqs. (1), (2) and (3) satisfy not only the geometrical conditions of the problem, but likewise another problem of numbers, in which x and y are both negative at the same time.

22. The conclusions arrived at concern-

ing the solution of problems may then be summed up as follows:

1° If the conditions of the successive problems to which we refer the proposed are reciprocal, in order, beginning with the proposed, the solutions involve each, the others.

If the successive conditions, in order, are not reciprocal, then:

2° If some of the conditions relative to any one whatsoever of these problems are simply consequences of those of the *following*, all the solutions of any one whatsoever are solutions of the proposed, but may not include them all.

3° If some of the conditions of any one are consequences of the *preceding*, so that what satisfies the latter will satisfy the former, the solutions of any one whatsoever comprehend all those of the proposed, and may include strange solutions.

If several of these cases hold successively in the solution of the problem we apply the proper principles in turn to each case.

23. THE SYNTHETICAL METHOD.—By this method, for the demonstration of theorems, we start with known propositions and deduce the others successively as consequences until we arrive at the theorems proposed, which is thus recognized as true.

Similarly in the resolutions of problems we start with a known solution and deduce a new one from it, a new one from this and so on, until we reach the proposed, whose solution is thus obtained.

24. The synthetical method is generally used in communicating results to others, and often in discovery, whether deducing consequences from known results at random, or by intelligently combining known results with a view to a certain end. As it is often difficult to know from what known problem or theorem to start, we can often make a series of fruitless essays before hitting upon the proper solution.

The analytical method possesses the advantage of a starting point, as we know from what result we have to go to seek one from which it is to be deduced; and this is often a great advantage, even in communicating results; for by the synthetical method, the reader often sees no object in the various steps of the demonstration or solution, until the end is attained.

No rules can be given as to the best method to employ—sometimes both may be employed in the same investigation—for by *deducing* certain results having more or less analogy to the proposed, we stand some chance of applying them in the course of the analytical march.

25. The ancient authors did not perfect the analytical method. Thus Euclid and Pappus both regarded a proposition as demonstrated when they could deduce from it a proposition known to be true. The examples solved by them carry out this idea, which is evidently defective, since we have seen that false premises can sometimes lead to true conclusions. After reaching the true proposition, if they had then reversed the order and verified the preceding results synthetically, the results would be proved true; or still better, if they had shown the reciprocity in the successive theorems or in the conditions of successive problems, the results would be true without the necessity of a synthetical verification. It is little wonder that the analytical method should be treated with so little favor, when its limitations and requirements were so imperfectly perceived.

26. HOW TO DEMONSTRATE THAT A PROPOSITION IS FALSE.—If we can deduce by just reasoning from a proposition, temporarily admitted, a false proposition, or one *incompatible* with any of the preceding, the assumed proposition is false; for if the first was true, all those deduced from it would necessarily be true, and could not consequently offer any incompatibility with it or any of the others.

This method is analytical, in that it takes for a point of departure the proposition in question and goes to some desired result, whose truth or falsehood thus establishes that of the first proposition.

27. REDUCTIO AD ABSURDUM.—Two propositions, of which each is the simple negative of the other, are contradictory (art. 6), and consequently, if one must be true, the truth of one involves the falsity of the other and reciprocally; so that if we can prove that the contradictory proposition to one is false, that one will be demonstrated true, provided that every contradictory case is stated. This method, called the "reductio ad absurdum," simply amounts to this:

Let A, B and C represent *all* the possible cases of a certain proposition, *one*

of which must be true. Now if we can prove that cases B and C are false (art. 26), then it must follow logically, that A is true, for one must be true, and case A is the only one not discarded as false. The latter method of putting the case is seized much more readily by students than the former, and amounts to the same thing as B and C are here contradictory to A, so that the first demonstration applies with the same results in both cases.

We have now given a rapid resumé of the laws of reasoning, and will now proceed to their application to geometry.

Application of General Methods to Geometry.

28. We are met at the threshold of the science of geometry with difficulties relative to proper definitions of the straight line and plane.

Euclid defines a straight line as one which rests evenly on its points, and adds, as an axiom, that two straight lines cannot enclose a space. The definition gives but little meaning, and should be rejected.

Modern authors define a straight line as the shortest distance between two points, and assume that any two straight lines can be made to coincide, besides often giving as an axiom a third property, that a straight line has the same direction throughout its whole extent, without inquiring whether some of these requirements may not be incompatible. Duhamel evidently takes a hint from Euclid's axiom, and defines *a straight line as such that through any two points we can only pass one straight line.* It follows that such a line will coincide with any other straight line when it has two points in common, and that the form of the line is everywhere the same.

This is a precise definition, but it does not immediately give us an idea of the form of the line meant by it.

If we define a straight line as one having everywhere the same direction, we form an idea at once of the kind of line meant, though the word direction must then be defined, as it is often associated with the path of a certain course on the earth's surface. It would be more precise to define *a straight line as one every point of which is in line with any two points of the line assumed at pleasure.* We sight along a straight edge to see if it is "straight," or satisfies the above condition, and this is really the first and most lasting impression we get of a straight line. Something has to be assumed as the result of observation or experience, no matter what definition is used. If we employ the last definition then it follows as a consequence that but one straight line can be drawn between any two points, which thus includes Euclid's axiom as a consequence, as well as Duhamel's definition, and the further fact that two indefinite straight lines coincide throughout their whole extent when they have two points in common. Accepting one of these definitions it should be next in order to *prove* that a straight line *is* the shortest distance between two points, and not assume it, as is done by the followers of Legendre. We shall enter into this demonstration directly, after defining a plane and showing some of its characteristics.

29. The usual definition given of a plane—such a surface that a straight line connecting any two of its points will lie wholly within or on the surface—is certainly the one derived from experience, for on looking over a smooth sheet of water we notice that it looks straight from point to point. Any one trying a straight edge on a blackboard will rest satisfied that there is such a surface, so it may be pertinently asked why not accept the definition, since we know that there is just such a surface as is described by it.

There can be no objection, that I see, from a practical point of view, but it is very desirable in pure theory to assume as little as possible, especially if the thing can be deduced from previous assumptions and axioms.

Therefore Duhamel conceives a plane to be generated by a straight line revolving about a point in another straight line and remaining always perpendicular to it. It is very evident that such a surface can exist, and we have only to show that a straight line passing through any two points of it lies wholly within the surface.

To prove this, conceive the surface completely turned over, so that the fixed straight line shall be in continuation of its first position, then if the point, the intersection of the perpendiculars to the

fixed line, is the same for the two surfaces they must necessarily coincide, since both are the loci of perpendiculars to the same fixed line at the same point. [For the same reason, if we revolve either surface about the fixed line it will coincide throughout with the other surface, though this fact is not necessary to the demonstration below.]

Now pass a straight line through *any* two points of the common surface, it will coincide with the surface throughout its whole extent. If you deny it, then it must leave the surface somewhere, either on the one or the other side; but any reason that may be given for its leaving the first surface on the side of the fixed line or the reverse, will apply equally well to the second surface, so that the line would have to be on both sides of the common surface at the same time, since the fixed line (or directrix) for one surface is on the opposite side of the common surface to that for the other surface.

Hence, as the straight line drawn through any two points of the plane must either lie in it entirely *or not*, and we have found the latter supposition absurd, the fact must hold (article 27).

It must follow, likewise, that any indefinite straight line which passes through any point outside of this locus (the plane surface), can have not more than one point in common with it, for if it had two, all its points would coincide with it, which is contrary to the hypothesis.

30. From the property just proved of the plane we can easily show that any two planes having any three common points, not in the same straight line, will coincide throughout. For, draw a straight line through two of the points, it will coincide with both surfaces throughout, and if we conceive an indefinite straight line to revolve about the third common point, always touching the line drawn, it will coincide with *both* surfaces at the same time in all its positions, since it has two points in common with them; therefore the two planes present a common surface, and coincide throughout.

31. If preferred, a plane may be defined as a surface generated by revolving a straight line about a fixed point, and always touching a fixed straight line not passing through the point. For, reversing the surface, and reasoning as before (29), we prove that a straight line will coincide with it from point to point, whence the conclusion of art. 30 holds.

32. We have given the reasons why the definition, that "the straight line is the shortest between two points," should not be accepted. It is a thing to be proved in due course, and we shall now give the few theorems, necessary to prove it and which should form some of the first theorems of any treatise. The demonstrations are taken mainly from Euclid, who did not fall into the error of modern geometers of assuming the principle in question when it was susceptible of proof. As the reader is supposed to be versed in geometric processes the demonstrations will be made very brief.

33. Using Duhamel's definition, that a straight line is such that but one straight line can be drawn through two points; or if preferred, the other that I suggest, that a straight line is such, that every point of it is in line with any two points of it whatsoever, which includes the above definition, we easily show in the usual manner, that,

If two straight lines cross each other, the vertical or opposite angles are equal.

Also, by superposition, it is shown in the usual manner that,

Two triangles are equal when they have an equal angle comprehended between two sides equal each to each.

34. Next we shall show, that,

The exterior angle of a triangle is greater than either of the opposite interior angles.

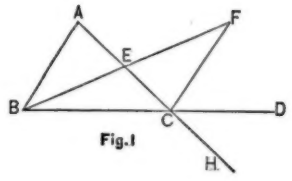

Fig.1

Let A B C (Fig.1) be a triangle, and ACD the exterior angle that we wish to prove greater than either BAC or ABC. Take E the middle of AC, draw BE and extend it to F, a distance EF = BE. Then the triangle AEB is equal to the triangle CEF (article 33) having the opposite angles at E and including sides, equal each to each. Therefore the angles A and ACF are equal; but as ACF

is less than ACD, from the construction, it follows that angle ACD is greater than the opposite interior A.

In a similar manner, by bisecting BC, &c., we can prove that the exterior angle BCH is greater than ABC; therefore, since BCH=ACD, (33), we conclude that any exterior angle is greater than either of the opposite interior, Q.E.D.

35. We shall next show that,

In an isosceles triangle, the angles opposite the equal sides are equal.

In the triangle ABC, let AB = AC (Fig. 2),

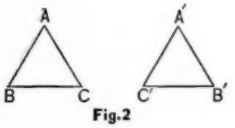

Fig.2

Turn the triangle ABC over, and suppose it to take the position A′B′C′, the angle B falling at B′, C at C′. Now slide the second triangle so as to coincide with ABC, which it can be made to do completely, since the angles A and A′ are equal, side AB=A′C′ and AC = A′B′. Therefore, the angles B and C′=C, are equal, which was to be proved.

36. *If two sides of a triangle are unequal the angle opposite the greater will be the greater.*

Let AC>AB (Fig. 3); take AD=AB, the triangle ABD will be isosceles, and

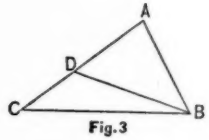

Fig.3

consequently, the angles ABD and ADB will be equal (35). But the angle ABC>ABD=ADB; and ADB>C (34): then for a stronger reason, ABC>C, which was to be proved.

37. *If two angles of a triangle are unequal the greater side will be opposite the greater angle.*

There are only three possible cases and one of them must be true; either the side opposite the greater angle must be less than, equal to or greater than the other side.

If it was smaller, the angle opposite would be the smaller of the two (36), which is contrary to the hypothesis.

If it was equal to the other side, the opposite angles would be equal (35), which is likewise contrary to the hypothesis. These two suppositions being false, the only remaining one, which agrees with the enunciation, must be true.

38. *In any triangle, any side whatever is smaller than the sum of the two other sides.*

In the triangle ABC (Fig. 4), prolong BA so that AD=AC; the triangle CAD will be isosceles and the angles D and

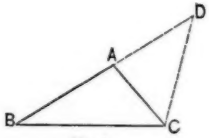

Fig.4

ACD will be equal; it follows that BCD>D, and consequently, in the triangle BCD the side BD will be greater than BC (37). But BD=BA+AC, hence the sum of the two sides BA, AC of the triangle ABC is greater than the third. Q.E.D.

39. Corollary.—It results from this, *that a straight line is shorter than any broken line having the same extremities.*

In Fig. 5, we wish to prove, that the straight line AB is shorter than the broken line AEDCB. Draw the diagonals AC, AD, then by (38)

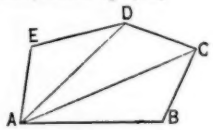

Fig.5

$$AB<BC+AC$$
$$AC<CD+DA$$
$$AD<DE+EA$$

Adding and cancelling like terms we find that,

$$AB<BC+CD+DE+EA$$

which was to be proved.

We cannot take up the subject of limits, in this short discussion; but regarding any curved line as the limit of any inscribed broken line, as the length of

each side becomes smaller and the number of sides increases indefinitely, we easily prove from the result above that a straight line is shorter than any curved line having the same extremities. So that we can state, generally, as a deduction from known principles, that *a straight line is the shortest between two points.*

40. It is evident now that there is no simplification, even in Legendre's method, of assuming the principle just proved; for the propositions necessary to prove the principle are theorems that should be proved in any case, hence we repeat that the unnecessary assumption should be rejected.

In fact it is very desirable to deduce all the theorems in geometry from the fewest possible assumptions, and those, too, so evident that every one recognizes their truth without hesitation; and this was plainly the aim of the older writers, who were obviously impelled to it by the many objections of the sophists; who thus incidentally assisted in making geometry as perfect a science as it is.

It would lead us too far to allude to some other minor objections to existing discussions of certain geometrical principles, we therefore resume the consideration of the application of the general methods previously exposed.

41. The proof for the theorems just given follows the synthetical method, which gives the results so quickly that nothing better can be desired.

We shall give some applications now of the analytical method, which in many cases it is more desirable to follow than the other.

THEOREM. *Similar triangles are to each other as the squares on their homologous sides.*

We are to prove that the areas, or half the products of the bases by the altitudes, are in this ratio.

Call a and h, respectively, the base and altitude of one triangle; a' and h', the base and altitude of the other. Then we are to prove the following proportion.

$$ah : a'h' :: a^2 : a'^2,$$

which entails the following, on cancelling the common factors,

$$h : h' :: a : a';$$

and, *reciprocally*, since the first proportion can be deduced from the last.

Now the last proportion is true, since in two similar triangles the altitudes are as the sides, therefore the theorem announced is true (Article 14), since it can be synthetically demonstrated from the last mentioned known result.

Contrast the usual demonstration by deduction, as given in the books.

42. The following little exercise is taken, almost at random, from a textbook.

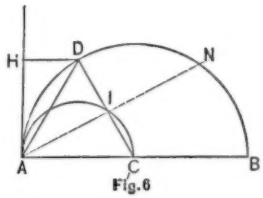

Fig. 6

Having described the semicircle AIC, on half the diameter of semicircle ABD, as shown in Fig. 6, and drawn the line CD from the center of ADB to any point D on its circumference, intersecting the smaller circumference at I, to prove that DI=DH where DH is the perpendicular from D to the common tangent at A.

Now, by analysis, we start with the relation DI=DH, we have to prove, and refer it, if possible, to another easier to prove. Thus we see at once, drawing the line AIN through I to intersection with ADNB, that AID is a right angle, being equal to AIC, an angle inscribed in the semicircle AIC; so that the two right triangles AHD and AID, having a common hypothenuse, would be equal (whence DH would equal DI) if we can only prove that the acute angles HAD and DAI are equal. We thus refer the proof of the original proposition to proving the equality of two triangles, and this in turn to proving the equality of two angles, and it is plain that the propositions are reciprocal.

Now the angles HAD and DAI are equal, the first being measured by one-half the arc AD, the second by one-half the arc DN; which arcs are equal, because CD being perpendicular to the chord AN bisects the whole arc ADN at D. Therefore the proposition is proved.

This going from the thing to be proved to known relations is much more natural, in the march of discovery, than the synthetical method in this case. By

the latter method of deduction we should first notice all the relations that had been previously proved about the lines in the figure, when by combining certain of them we could deduce the theorem.

The contrast in the two methods is much more apparent, though in more complicated exercises, where a number of successive steps have to be made, but we cannot enter into them.

43. There are certain other *methods* sometimes used in the resolution of problems.

It is sometimes easier to construct a system *like* the one we wish, and afterwards reduce it to the proper dimensions; or it may be desirable to construct a part of the new system by itself to an assumed scale and complete the construction by putting the given lines in to the same scale and afterwards reducing.

The modern graphical statics and its applications afford many examples.

44. Another expedient is the *method of geometric loci*, or finding the position of a point by tracing a line, which is the locus of the point when one condition of the problem is omitted, and noting its intersection with another line on which the point should be found likewise. The following problem is an example, though it is not the best solution of this particular problem.

Given the base = BC (Fig. 7), the length of the altitude=MH, and length =AN, of the line bisecting the angle opposite the base, from the vertex to the base, to construct the triangle.

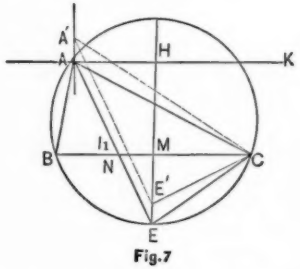

Fig.7

If we draw a right-angled triangle with the altitude for one side, and the length AN for the hypothenuse, we get at once the inclination I of this bisector to the base. The problem is to find the vertex A. It is, of course, on the line HK parallel to the base, and at the distance HM from it. Suppose the problem solved, and that ABC is the required triangle, about which circumscribe a circle, then extend AN to E, its intersection with the circumference, and draw EH perpendicular to BC, intersecting it at M. The last named point is the middle of BC, since arc BE=arc EC, being double the measure of the equal angles BAE and EAC respectively. Also, angle I=angle ACE, since double their measures are arcs (AB+EC) and (AB+BE) respectively, which arcs are equal; therefore it looks probable that the last relation may give a locus for point A.

Thus, from some point E′ on the indefinite line HME′, passing through the middle of BC and perpendicular to it, draw E′A′, making the required angle I with the base, and lay off at C the angle E′CA′=I; the intersection of the dotted lines A′, will give a vertex for a triangle BA′C (not drawn entirely) that satisfies every condition but that of altitude of the required triangle.

For conceive a circle to pass through B,E′ and C; it will likewise pass through A′, otherwise it will cut the lines E′A′ and CA′ in some points F and G.

Then, since angle E′CA′ was laid off equal to I, their respective measures are equal; whence

$$arcs\ GB+BE'=FB+E'C,$$

$$arc\ GB=arc\ BF.$$

That is, the points F and G must coincide, which is only possible when they are identical with A′, therefore the circumference that passes through BE′ and C will likewise pass through A′, hence we see that A′E′ is a bisector of the angle BA′C, so that triangle BA′C satisfies every condition but height.

It follows that taking other points as E′ on HE, and constructing the corresponding angle at C equal to I, and finding the intersection of the lines such as E′A′ and CA′ that we have a locus A′. . . for the vertex with one condition omitted. The intersection of this locus with HK produced gives A the vertex required.

45. *Application of Algebra to Geometry. Equations corresponding to Geometric Loci.*

We shall now, rather abruptly, enter the domain of co-ordinate geometry

whose origin is due to Descartes, at least when the science is considered in all its generality.

Geometric loci are here represented by an equation between the co-ordinates of each of its points, these co-ordinates being supposed to be laid off in opposite directions when they have unlike signs. Thus if $+x$ represents a distance laid off to the right, from a given origin, along a certain line; $-x$ will represent the same distance, laid off to the left, from the same origin, along the given line, a conception that forms the base of the modern algebra of algebraic numbers. Unfortunately Duhamel has made no use of this algebra, and thus is constrained to remark, after verifying the generality of an equation for both + and — values of the variables, that from an algebraic point of view, there is no sense in these operations upon minus quantities; but that applying the usual rules of multiplication of polynomials, etc., to them, they can be treated as real quantities, so far as giving the greatest generalization to the results is concerned.

This limited view of the subject we do not concur in, and refer to modern treatises on algebra, or to a previous article on algebra, by the writer, where the theory pertaining to the algebraic series of numbers is logically developed from comprehensive definitions given to + and — in connection with elementary principles.

However, this very generality, obtained by conceiving the variables to have both plus and minus values, in the equations of geometric loci, should cause the greatest care to see that no strange solutions are introduced by too great generality, or that none are lost by introducing too many restrictions. These discussions are sometimes difficult, but if we neglect them we are not completely assured of the result.

It is with such interesting discussions that we purpose to continue the subject of the application of general methods previously exposed.

46. Let us propose first to find *the equation of the locus of points such that the ratio m of their distances to a point and a given straight line may be constant*, a condition expressed, for the following figure, by the equation,

$$AM = m.\ MP. \quad . \quad . \quad (1).$$

A representing the given point, and BD the given straight line.

Assume A as the origin, and let the axis of x be perpendicular to BD, the axis of y parallel to it, and call the distance $AB = a$.

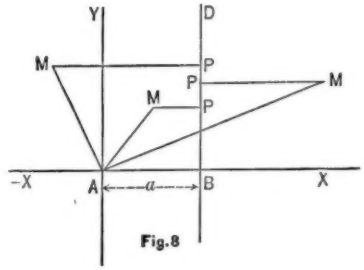

Fig.8

Now, whether x and y are + or —, that is, wherever the point M is, we have

$$AM = \sqrt{x^2+y^2}.$$

As to MP, when M lies between AY and BD, $MP = a - x$; when M is to the right of BD, $MP = x - a$; finally, when M is to the left of AY, since x is essentially negative, we write, $MP = a - x$, though $(a-x)$ here is really an arithmetical sum.

The condition, eq. (1), is then replaced by the two following, the first referring to points to the left of BD, the second to points to the right of BE:

$$\sqrt{x^2+y^2} = m(a-x)$$
$$\sqrt{x^2+y^2} = m(x-a),$$

whether the point M is above or below AX.

Now if we square these equations, they can both be expressed by a single one.

$$x^2+y^2 = m^2(a-x)^2 \quad . \quad . \quad (2),$$

which is thus the equation for the geometric locus required, for it includes every position of the point M, giving the corresponding value of y for any assumed value of x. There are thus no solutions lost; neither are there strange solutions; for in squaring the two equations above the result is the same if both radicals are negative as well as positive, as assumed, and the effect of changing the signs of the radicals above from + to — would simply transform the right

member of one equation into the right member of the other; so that the final result (eq. 2) will be the same as before; so that equation (2) of the locus contains no strange solutions.

47. *Equation of the locus of points such that the sum of their distances to two fixed points may be constant.*

Let F and F′ (Fig. 9) be the two points, $2c$ their distance apart, and $2a$ the constant sum.

Let us take for the axis of x the straight line FF′; for the axis of y the perpendicular to FF′, erected at its middle A, and call x, y the co-ordinates of any point M whatsoever of the locus.

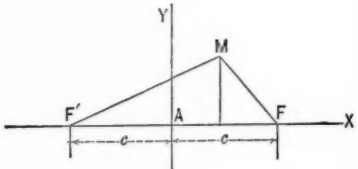

Fig. 9

The condition to which it is subjected will be expressed by the equation,

$$FM+F'M=2a,$$

or,

$$\sqrt{y^2+(x-c)^2}+\sqrt{y^2+(x+c)^2}=2a \quad (1)$$

the co-ordinates having signs corresponding to the position of the point M. The formula is evidently true for any of the positions of M.

On changing the second radical to the right member, squaring and reducing, we find,

$$a^2+cx=a\sqrt{y^2+(x+c)^2}$$

Squaring again and reducing, we have

$$a^2y^2+(a^2-c^2)\,x^2=a^2\,(a^2-c^2) \quad (2)$$

Now we should get the same equation, by this repeated squaring, no matter what the signs of the radicals in (1); so that although (2) comprehends the locus required it may contain some loci "strange" to the question.

To examine into the other solutions, we observe first that we must exclude the case of minus signs to both radicals at once; but one radical may be + and the other −, with the same result, eq. (2) above. *This corresponds to the difference of the two distances FM and F′M′*, no matter which is the greater.

We should naturally infer that (2) represents two loci at the same time, the one corresponding to the condition FM + F′M = 2a, the other to FM − F′M = 2a; but it is easily seen that this cannot be, for since FM + F′M > FF′ or $2a>2c$ or $a>c$, for the first locus, in the second we must have $2a<2c$, since in any triangle the difference of two sides is smaller than the third. Therefore if, $a>c$ we have, placing $a^2-c^2=b^2$

$$a^2y^2+b^2x^2=a^2b^2$$

for the equation of the first locus, called an *ellipse*, and if $a<c$, placing $c^2-a^2=b$,

$$a^2y^2-b^2x^2=-a^2b^2$$

for the equation of the locus of points such that the difference of their distances from F and F′ may be $2a$, a curve known as the *hyperbola*. We thus have incidentally found the equation of another locus than the one corresponding to the original enunciation. We see plainly that it is introduced by the elimination of radicals, so that eq. (2) the so-called "consequence" of (1) (see articles 19 and 20) is really more comprehensive and includes more than the consequence of the original enunciation.

48. *Equation of the locus of points such that the sum of their distances to a point and a fixed straight line may be constant.*

Let F and UV (Fig. 10) be the point and straight line respectively; AX, perpendicular to UV, the axis of x, and AY perpendicular to AX, the axis of y, drawn through a point A sufficiently far, so that all the points M that we shall consider may be on the same side of this axis.

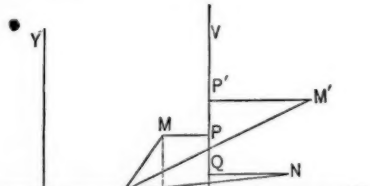

Fig. 10

Let x and y be the co-ordinates of any point M of the locus, situated between UV and AY; m the given sum, FB$=a$ AF$=b$, the condition of the question is expressed by the equation FM$+$MP$=m$, or,

$$\sqrt{y^2+(x-b)^2}+(a+b-x)=m \quad (1)$$

and we see that we evidently have $m>a$ for this case. But if we transpose $(a+b-x)$ to the right member and square both sides we see that we shall obtain the same result, if eq. (1) was written,

$$-\sqrt{y^2+(x-b)^2}+(a+b-x)=m,$$

which corresponds to the geometric locus, expressed by the condition, MP$-$FM $=m$; for which, however, we evidently always have $m<a$. Rationalizing, either equation reduces to the following,

$$y^2-2(m-a)x=(m-a)^2-2b(m-a) \quad (2)$$

which is the equation of a *parabola*, which thus evidently corresponds to two loci; the one when $m>a$ answering to the original problem, the other when $m<a$, corresponding to a new geometric problem. But so far, we have only considered points to the left of UV. For points to the right $x>a+b$ and the first member of (1) becomes the difference FM′$-$M′P′. Calling this difference m, we have for points to right of UV, $m>a$, as is evident from the figure. Equation (2) which embraces this locus, also corresponds to the case when the radical is minus, *i. e.*, FM′ $-$M′P′$=-m$; but as this has no sense, we exclude it.

Equation (2) thus corresponds to two geometric loci for points to left of UV, according as m is greater or less than a, and one locus for points to right of UV, which requires that m be greater than a. As this equation then, for points to right of UV, does not correspond to the original enunciation, let us find the equation that does. Calling x, y the co-ordinates, of a point N such that FN $+$ NQ $=m$ there results,

$$\sqrt{y^2+(x-b)^2}+x-a-b=m \quad (3)$$

which becomes,

$$y^2+2(m+a)x=(m+a)^2+2b(m+a) \quad (4)$$

It is easy to see that eq. (3) which satisfies the geometrical condition where $x>$AB, does not satisfy it, if we take $x<$ AB, for then the first member becomes the difference FM$-$MP, which corresponds to the second case mentioned as belonging to eq. (2).

The problem proposed cannot then be completely resolved, except by two equations, and each of the two represent loci, of which a part belongs, whilst the other does not belong to the question, and yet it is impossible to separate them.

This interesting example should teach caution in assuming that an equation deduced to represent the position of certain points is necessarily general or corresponds to all points of the locus.

49. In the preceding cases the strange solutions have been introduced by the elimination of radicals. They may likewise be found when an equation contains a term of the form $\frac{F(x,y)}{\varphi(x,y)}$, and we multiply both members by $\varphi(x,y)$. The new equation will admit necessarily of values that annul $\varphi(x,y)$ and $F(x,y)$ at the same time. If these solutions pertain to the question, there are no strange solutions introduced by the multiplication; otherwise there will be, and it will be impossible to suppress them.

A case in point is the equation of the conchoid.

$$\frac{y}{\sqrt{y^2+x^2}}=\frac{y-a}{m} \quad . \quad . \quad (1)$$

Making $x=o$, we find $y=a\pm m$; but if we clear of fractions, the values $x=0$, $y=o$ satisfy the equation, but not the geometric condition (as is well known), and is thus strange to the question. The left member of eq. (1) takes the form $\frac{0}{0}$ for these values.

50. *Locus of all the points from which we can view, under the same angle, a straight line of given length and position.*

Let BB′ (Fig. 11) be the given straight line, $2a$ its length, and t the tangent of the angle under which it must be viewed from any point M of the locus.

Take BB′ for the axis of x, and the perpendicular erected at its middle for the axis of y.

The equations of the straight lines drawn from B and B′ to M, calling m and m' the tangents of a and a' and a and a, the intercepts on the axis of x, are,

$$y=m(x-a),\ y=m'(x+a).$$

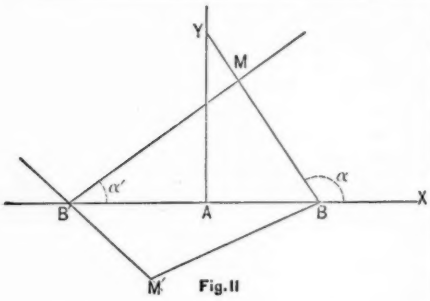

Fig. II

If the point M is above the axis of x, the angle under which BB will be seen is $(\alpha-\alpha')$, and we have

$$t=\frac{m-m'}{1+mm'} \quad . \quad . \quad . \quad (1)$$

But if the point is at M′, below AX, the angle under which BB′ will be seen will be $(\alpha'-\alpha)$, whose tangent is,

$$\frac{m'-m}{1+mm'}=-t.$$

The points of the plane *satisfying* (1) will be such then, that from those above AX we shall see BB′ under the angle whose tangent is t, whereas from those below AX, BB′ will be seen under the angle whose tangent is $-t$, that is under the supplementary angle. We make this explanation now, as we necessarily include this result in making use of the single equation (1), and cannot get rid of it. Finding the values of m and m' from the equations of the straight lines and substituting in equation (1) and reducing, we find,

$$y^2+x^2-a^2=\frac{2ay}{t};$$

which can be put under the following form,

$$\left(y-\frac{a}{t}\right)^2+x^2=a^2\left(1+\frac{1}{t^2}\right) \quad . \quad (2)$$

which we recognize at once as the equation of a circle, whose centre has $x=o$, $y=\frac{a}{t}$, and whose radius is $=\frac{a\sqrt{(1+t^2)}}{t}$.

Its upper and lower parts are segments capable of supplementary angles, as we have foreseen.

The locus of points below AX, from which BB′ would be viewed under the given angle whose tangent is t, would correspond to eq. (1) if t were replaced in it by $-t$, and both the resulting equation and (2) above would correspond to circles, of which only a part belongs to the question proposed.

We have here an example of lost and strange solutions at the same time, for eq. (1) does not include the locus of *all* the points above and below AX, from which BB′ can be viewed under the same angle, so that by Article 17 there will be *lost* solutions; but we recognized at the start, also, that eq. 1 satisfies two different geometrical conditions for points above and below AX, so that all the solutions of eq. 1 are not solutions of the original problem or the conditions are not reciprocal, whence by the reasoning of Article 19 the *strange* solutions are accounted for.

We see here again how erroneous it would have been to have assumed that an equation deduced by considering one point only, M above the axis of x, was general and applicable to all points of the required locus.

An attentive consideration of the preceding examples, in connection with the exposition of general methods, previously given, shows that great care must be exercised, in endeavoring to find a single equation to represent a given geometric locus, in order to avoid lost or strange solutions, which will surely be found if the conditions of the two problems are not reciprocal.

51. The converse problem, that of finding the geometric locus corresponding to a given equation, is often simpler, for here there are no lost or strange solutions, but only those that can regularly be deduced from the equation; but even in this case we must consider both plus and minus values of the quantities that admit of them entering the equation; for when an equation is given we tacitly assume it to be general, unless it is expressly stated that it pertains only to certain values or quadrants or other limits. But this very generality, without which analytical geometry could not exist as a complete science, demands a corresponding circumspection in all the discussions pertaining to it.

This observation naturally suggests, certain remarks relative to formulæ expressed in terms of polar co-ordinates,

with which we shall conclude this discussion.

52. If we consider a point in a plane referred to rectangular co-ordinates x, y, the formulæ for passing to polar co-ordinates are

$$x = r\cos\varphi,\ y = r\sin\varphi \quad . \quad . \quad . \quad (1).$$

where r = radius vector from the origin of rectangular co-ordinates and φ, the angle made by it with the axis of x. Now, since *cos* φ is positive or negative with x, and *sin* φ positive or negative at the same time as y, it is evident that the above formulæ are general, and hold for any values of the angle φ, provided we take the signs of the functions established in trigonometry, and regard x and y as positive in one sense and negative in the other, r being regarded always as positive and in absolute value. But a little inspection of the formulas will show that if we put $(\varphi \pm \pi)$ for φ, and $-r$ for r, that the formulas will pertain to the same point, if we agree that r must be laid off in an opposite direction from the origin to the radius vector, since r and the trigonometric functions both change signs at the same time, so that the products giving the values of x and y will be the same as before. (Puckle, in his *conic sections*, page 8, has assumed this principle, without deriving it from any formulas, and given illustrations of its application.)

Now let the equation of a locus expressed in rectangular co-ordinates be

$$F(x, y) = 0;$$

its polar equation will be

$$F(r\cos\varphi,\ y\sin\varphi) = 0 \quad . \quad . \quad (2)$$

and we have seen that, taking positive values of r corresponding to any angle φ, eq. (2) will give all the points of the locus represented by the eq.

$$F(x, y) = 0;$$

which we should likewise find by substituting $(\varphi \pm \pi)$ for φ at the same time that we put $-r$ for r, since the same point of the locus is given by the co-ordinates $(\varphi \pm \pi)$, and $-r$ as by φ and $+r$, with the distinct understanding that all angles are to be laid off, going around in the same direction from the axis of x, and that $-r$ is to be laid off on the radius vector produced and of the same absolute value as $+r$. Thus we can either regard r as always positive, and vary φ from 0 to 2π to get all the points, or admitting negative values of r it will suffice to vary φ from 0 to π.

53. It is important to observe that if the original equation in rectangular co-ordinates F $(x, y) = 0$, was subjected to any restriction, that Eq. (2) will likewise be subjected to the same restriction.

To illustrate, suppose F (x,y) contains a radical of the form $\sqrt{mx^2 + ny^2}$ that must always be regarded as positive. Transforming this into polar co-ordinates by means of eqs. (1), the radical becomes,

$$+\sqrt{m r^2\cos^2\varphi + n r^2\sin^2\varphi},$$

and will remain the same when we put $\varphi + \pi$ and $-r$ in place of φ and r.

Now, if we had passed the factor r^2 outside of the radical and written it

$$r\sqrt{m\cos^2\varphi + x\sin^2\varphi},$$

we must always take r positively, for changing φ and r to $\varphi + \pi$ and $-r$, would give a minus radical. The double construction then only applies in this case when the radical can be taken with the double sign.

54. Again, when the transformed equation (2) can be decomposed into factors, the double construction will not apply necessarily to each factor separately, for it has not been so demonstrated.

Thus the polar equation of the ellipse, with the focus for a pole, is

$$(a^2 - c^2\cos^2\varphi)\, r^2 + 2b^2 cr\cos\varphi - b^4 = 0,$$

and we see that if r and φ satisfy the equation, $-r$ and $\varphi + \pi$ will likewise satisfy it; but if we decompose the first member into two factors, and put them equal to zero, we find the two equations,

$$r = \frac{b^2}{a + c\cos\varphi},\quad r = \frac{b^2}{-a + c\cos\varphi},$$

neither of which is satisfied when we replace r and ϕ by $-r$ and $\varphi + \pi$, although one is converted into the other.

In constructing the locus from either one of these equations, we notice that the first always gives positive values of r, whereas the second will always give negative values of r, since $a > c$; therefore if we use the last equation we must be careful to lay off the values of r in an opposite sense to those for $+r$. The

values of the last will agree with those pertaining to the first equation, when φ in the first corresponds to $\varphi+\pi$ in the second, as we have seen.

A similar investigation can be made for the hyperbola, with corresponding results.

55. Where the angle φ, and not its trigonometric function, enters an equation, the previous reasoning does not apply. Let us take a simple example, the *spiral of Archimedes*, and ascertain if any generalization can be effected by the use of minus radii vectors, and whether a double construction is possible.

Suppose a point moving uniformly along a straight line which turns uniformly around one of its points in a given plane.

Let us take for a pole the fixed point A, and for an axis AU; call AB$=a$, AM$=r$, and MAB$=\varphi$; then if b designates the amount the point moves along the straight line, whilst it turns an angle equal to unity, taking for the measure of the angles at the center the ratio of the arcs

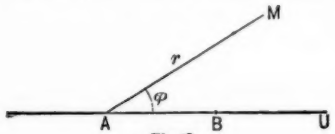

Fig. 12

intercepted to the radius, we have,

$$r=a+b\varphi \quad . \quad . \quad (1)$$

for the equation of the locus, if a = AB gives the value of r on AU when $\varphi=0$.

This equation holds for all values of φ from 0 to ∞. But can it represent the positions of the moving point anterior to its arrival at B? We can answer this question in the affirmative, if we agree that angles counted in the opposite direction to φ shall be minus, for then the equation above takes the form $r=a-b\varphi'$, if φ' is the absolute value of the angle reckoned to the right.

If this turning in the negative direction is continued, so that $b\varphi'$ becomes greater than a, the same equation suffices to represent the position of points in the indefinite past, before A was reached, provided we agree to lay off the minus values of r in an opposite direction to the plus values. The equation now takes the form $-r=b\varphi'-a$.

With the conventions established we see that negative values of r and φ serve to generalize formulas in equations where the angle, and not its function, enters; so that one formula, (1) in this instance, suffices to represent all the points of the locus.

But the double construction, previously mentioned, for the same point, does not hold in this case. Thus equation (1) is not satisfied when we change the coordinates φ and r to $\varphi+\pi$ and $-r$.

It is seen from the preceding investigation that a negative radius vector is not always admissible, but that when it is, there can be no possible objection to its use, especially if an investigation is thereby simplified in any way.

In fact the use of both plus and minus quantities, in the sense of opposition throughout the whole range of mathematics, allows a condensation in the expression of results, without which the science would be in a very imperfect state; but this very contrivance imposes a corresponding attention to every phase of a question, in order to establish the generality, when it exists, or the proper limitations when it does not exist.

It is hoped that the preceding exposition and illustrations of the methods to be used will prove of assistance in such researches.

In 1862 Count de Lauture tried to simplify the Chinese language so that it could be used for telegraphing, but he was unable to solve all the difficulties of the problem. In 1866 M. Viguier experimented with autographic telegraphs, and proposed to employ a cipher code, by which the 44,000 Chinese characters could be transmitted by the Morse apparatus. This code was first published in 1870, when a cable was laid between Shanghai and Hong Kong. Every Chinese character is composed of two parts—one is called the radical or key, the other is phonetic. Every Chinese character can be classed under one of 214 radicals. In his first essay Viguier used three numbers to represent each character—that of the radical, that of the column under the radical, and that of its order in the column. This system required the use of numbers varying from three to six ciphers, which rendered the transmission of despatches slow and difficult. By eliminating the characters which are rarely used, and employing those which occur in ordinary correspondence, he was able to simplify his system, so as never to require more than four figures for a single character. To this improved method he added Chase's system of holocryptic ciphers, so that messages can now be sent readily and with perfect secrecy.

RECENT PROGRESS IN DYNAMO-ELECTRIC MACHINES.

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From the "Journal of the Society of Arts."

I.

Fifteen months ago, I had the honor of delivering in this place a course of three Cantor Lectures on "Dynamo-electric Machinery." In the first of those lectures, the endeavor was made to trace out a physical theory of the action of dynamos, and to follow the theory into its bearings upon the construction of such machines. In the second lecture a large number of actual machines were considered and compared with one another and with the theory; and in the third lecture, the dynamo was considered in its functions as a mechanical motor.

As the present paper may be considered supplementary to the Cantor Lectures, it will be convenient to treat of the features of progress which come to-night under review in a similar order of topics. I, therefore, take up first the theory of the dynamo.

There are, in fact, three distinct theories of the dynamo: (1) a physical theory, dealing with the lines of magnetic force and lines of current in which these quantities are made, without further inquiry into their why or how, the basis of the arguments; (2) an algebraical theory, founded upon the mathematical laws of electric induction and of theoretical mechanics; and (3) a graphical theory, based upon the possibility of representing the action of a dynamo by a so-called "characteristic" curve, in the manner originally devised by Dr. Hopkinson, and subsequently developed by Frölich, Deprez, and others. The last of these three methods, though it has not received any great or striking development during the past year, has proved itself to be the most invaluable aid in the practical construction of dynamo-machines. One has only to refer to the use made of characteristics by Mr. Kapp in his articles on the winding of "compound" dynamos, and, still more recently, by Dr. Hopkinson, in solving certain problems in the electric transmission of energy, to see how invaluable the method is.

In the algebraic theory much progress has been made during the past year; and there certainly was room for it. Monsieur Joubert has published, in the *Journal de Physique* for July, 1883, a long mathematical article, the object of which is to deduce the equations of the dynamo, taking into account not only the action of self-induction in the circuit, but also some of the terms of the second order usually neglected in first approximations. It is a question whether he has not still omitted some terms of quite as great an importance as those retained in the complicated formula deduced by him. But the matter can hardly be discussed in the present paper. Still more recently, Professor Clausius has published in Wiedemann's *Annalen*, for November last, a paper expounding a mathematical theory of dynamo-electric machines far more comprehensive, and, I venture to say, far more true, than any other yet put forward. Without shirking any of the mathematical difficulties presented by the complications of mutual induction between, and self-induction in, the various organs of the machine, and by the admitted incompleteness of all our formula for connecting the magnetism of an electro-magnet with the strength of its exciting current, Prof. Clausius has succeeded in putting the equations into a shape, not only far more satisfactorily from the point of view of completeness, but in framing those equations in a manner that must commend itself to every engineer. The relative simplicity attained by Clausius is, in fact, due to his lavish introduction of a set of arbitrary constants, each one of which having values that must be determined by experiment, for each machine or type of machines. The number of new symbols thus introduced is considerable; and it would be very desirable to find names for the separate constants to be determined. An excellent translation has appeared in the *Philosophical Magazine* of this year, and another is in process of publica-

tion in *The Electrician*, in which journal the series of articles on the theory of dynamo-electric machines from the pen of Professor O. J. Lodge is still continued. It cannot be said even yet that the mathematical theory of the dynamo is near completion. A further paper by Professor Clausius is promised; and it remains to be seen whether this article will deal with some of those points in which the graphic method has been so useful in practice, for example, in determining the proper quantities of wire for the coils in "self-regulating" or "compound" machines, and in finding the best shape to give to magnets and pole-pieces.

Turning to the physical theory of the dynamo, there is much to record. Our knowledge of the inductive actions which go on between the field-magnets and armatures of dynamos, has received considerable additions during the past year from the researches of Isenbeck, Cunynghame, Pfaundler and others. There is a good deal to be said on this head, and I have several new results to announce as the result of my own observations. Let me take as my starting point a matter mentioned in my Cantor Lectures, namely, the distribution of potential round the collector or commutator of a dynamo. Mr. W. M. Mordey, who first drew my attention to the fact that this distribution was irregular in badly-designed machines, had devised the following method of observing it. One terminal of a voltmeter was connected to one of the brushes of the dynamo, and the other terminal was joined by a wire to a small metallic brush or spring, which could be pressed against the rotating collector at any desired part of its circumference. I then made the suggestion that these indications might with advantage be plotted out round a circle corresponding to the circumference of the collector. Figs. 1 and 2, which are reproduced from my Cantor Lectures, serve to show how the potential in a good gramme machine rises gradually from its lowest to its highest value. The same values as are plotted round the circle in Fig. 1 are plotted out as vertical ordinates upon the level line in Fig. 2. I made the remark at the time that if the magnetic field in which the armature rotated were uniform, this curve would be a true "sinusoid," or curve of sines; and that the steepness of the slope of the curve at different points would enable us to judge of the relative idleness or activity of coils in different parts of the field. About the same time, I developed this method of observation a little further, and used two small metal brushes, at a distance apart equal to the width between the middle of two consecutive bars of the collector of my little Siemens dynamo, for the same purpose.* As the collector rotated, these two little brushes (see Fig. 3) gave on the voltmeter an indication which measured exactly the activity of the induction, in that section of the armature which was passing through the particular position in the field corresponding to the position of the contacts. I found, in the case of my Siemens dynamo, that the result was fairly satisfactory, for the difference of potential indicated was almost *nil* at the sections close to the proper brushes of the machine, and was a maximum about halfway between. In fact, the differences of potentials was rising most markedly at 90° from the usual

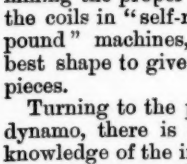


Fig. 1

DIAGRAM OF POTENTIAL ROUND THE COLLECTOR OF GRAMME DYNAMO.

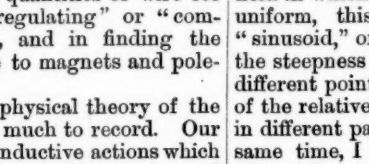


Fig. 2

HORIZONTAL DIAGRAM OF POTENTIALS AT COLLECTOR OF GRAMME DYNAMO.

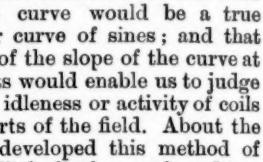


Fig. 3

METHOD OF EXPERIMENTING AT COLLECTOR OF DYNAMO.

* Dr. Isenbeck has also independently used a similar arrangement to investigate the induction going on in a Gramme dynamo.

brushes, or precisely in the region where (as seen in Fig. 2) the slope of the curve of total potential was greatest. One immediate result of Mr. Mordey's observations on the distribution of potential, and of my method of mapping it, may be recorded. I pointed out to Mr. Mordey that in a dynamo where the distribution was faulty, and where the curves of total potential showed irregularities, the fault was due to irregularities in the induction at different parts of the field; and that the remedy must be sought in changing the distribution of the lines of force in the field by altering the shape of the pole pieces. I am able now, after the lapse of fifteen months, to congratulate Mr. Mordey on the entire and complete success with which he has followed out these suggestions. He has entirely cured the Schuckert machine of its vice of sparkling. The typical bad diagram given in my Cantor Lectures was taken from a Schuckert machine before it received from his hands the modifications which are so signally successful to-day.

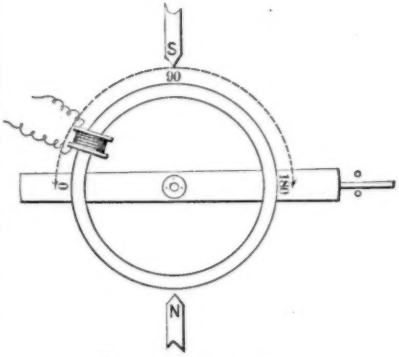
Since the experiments above detailed, I have experimented on my Siemens dynamo in another way. The machine was dismounted, and its field magnets separately excited. Two consecutive bars of the collector were then connected with a reflecting galvanometer having a moderately heavy and slow moving needle. A small lever clamped to the collector allowed the armature to be rotated by hand, through successive angles equal to 10°, there being thirty-six bars to the collector. The deflexions obtained of course measured the intensity of the inductive effect at each position. The result confirmed those obtained by the method of the two wire brushes.

I mention these methods, which have been used in my laboratory at Bristol, and have not been published before, because they relate strictly to the physical theory of the dynamo as developed in my Cantor Lectures, and also because of their practical application to all dynamos in which any such defect appears. They are also very closely related to the researches of Dr. Isenbeck, which next claim attention.

Dr. Isenbeck described, in the *Electrotechnische Zeitschrist* for last August, a beautiful little apparatus for investigating the induction in the coils of a Gramme ring, and for investigating the influence exerted by pole pieces of different form upon these actions.

Isenbeck's apparatus (Fig. 4) consists of a circular frame of wood placed between the poles of two small bar-magnets of steel, each 25 centimeters long, lying

Fig. 4



ISENBECK'S APPARATUS.

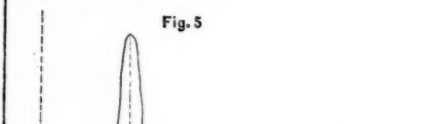
25 centimeters apart. On the frame which is pivoted at the center, is carried a ring of wood or iron, upon which is placed at one point a small coil of fine wire. This corresponds to a single section of the coils of a Pacinotti or of a Gramme ring, of which the ring of wood or iron constitutes the core. The coil can be adjusted to any desired position on the ring, and the ends communicate with a galvanometer. On vibrating it isochronously with the swing of the needle of the galvanometer, the latter is set in motion by the induced currents, and the deflexion which results shows the relative amount of induction going on in the particular part of the field where the coil is situated. The vibrations of the frame are limited by stocks to an angle of 7° 5′. Pole pieces of soft iron, bent into arcs of about 160° so as to embrace the ring on both sides, but not quite meeting, were constructed to fit upon the poles of the magnets. In some of the experiments a disc of iron was placed internally within the ring; and in some other experiments a magnet was placed inside the ring, with its poles set, so as either to reinforce, or to oppose, the action of the two external poles. In Dr. Isenbeck's hands this apparatus

yielded some remarkable results. Using a wooden ring, and poles destitute of polar expansions, he observed a very remarkable inversion in the inductive action to take place at about 25° from the position nearest the poles.

Fig. 4 is a sketch of the main parts of Isenbeck's instrument, and shows the small coil mounted on the wooden ring, and capable of being vibrated to and fro between stops. When vibrated at 0°, or in a position on the diametral line at right angles to the polar diameter, there is no induction in the coil; but as the coil is moved into successive positions round the ring towards the poles, and vibrated there, the induction is observed first to increase, then die away, then begin again in a very powerful way, as it nears the pole, where the rate of cutting the lines of force is a maximum. This powerful induction near the poles is, however, confined to the narrow region within about 12° on each side of the pole. It is beyond these points that the false inductions occur, giving rise in the coil, as it passes through the regions beyond the 12°, to electromotive forces opposing those which are generated in the regions which are close to the poles.

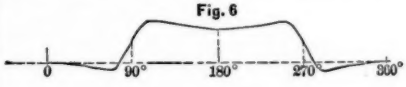
These inverse inductions were found by Isenbeck to be even worse when an iron disc or an internal opposing magnet was placed within the ring; but a reinforcing magnet slightly improved matters. Of course such an action in a Gramme armature going on in all the coils, except in those within 12° of the central line of the poles, would be most disastrous to the working of the machine; and the rise of potential around the collector would be anything but regular. In Fig. 5 I have copied out Isenbeck's curve of induction for the consecutive four quadrants. From 0° to 90° the exploring coil is supposed to be vibrated in successive positions from the place where, in the actual dynamo, the negative brush would be, around to a point opposite the S pole of the pointed field-magnet. From 90° to 180° it is passing around to the positive brush; from 180° to 270° it passes to a point opposite the N pole; and from 270° to 360° returns to the negative brush. Now, since the height of this curve, at any point, measures the induction going on in a typical section as it moves through the corresponding region of the field, and since in the actual Pacinotti or Gramme ring the sections are connected all the way around the ring, it follows

Fig. 5



that the actual potential at any point in the series of sections will be got by adding up the total induced electromotive force up to that point. In other words we must integrate the curve to obtain the corresponding curve of potential corresponding with the actual state of things around the collector of the machine. Fig. 6 gives the curve as integrated expressly for me from Fig. 5 by the aid of the very ingenious curve integrator of Mr. C. Vernon Boys. The height of the ordinate of this second curve at any point is proportional to the total area enclosed under the first curve up to the corresponding point. Thus the height at 90° in the second curve is proportional to the total area up to 90° below the first curve. And it will be noticed that though the induction (first curve, Fig. 5) decreases after 90°, and falls to zero at about 102°, the sum of the potentials (second curve, Fig. 6) goes on increasing

Fig. 6



up to 102°, where it is a maximum, and after that falls off, because, as the first curve shows, there is from that point onwards till 180° an opposing false induction. If this potential curve were actually observed on any dynamo, we might be sure that we could get a higher electromotive force by moving the brush from 108° to 102°, or to 258°, where the potential is higher. Any dynamo in which the curve

of potentials at the commutator presented such irregularities as Fig. 6, would be a very inefficient machine, and would probably spark terribly at the collector. It is evident that the induction in some of the coils is opposing that in some of the adjacent coils.

Two questions naturally arise: Why should such detrimental inductions arise in the ring? and how can they be obviated? The researches of Dr. Isenbeck supply the answer to both points. Dr. Isenbeck has calculated from the laws of magnetic potential the number of lines of force that will be cut at the various points of the path of the ring. He finds that the complicated mathematical expression for this case, when examined, shows negative values for angles between 12° and 90°. The curves of values that satisfy his equations have minima exactly in those regions where his experiments revealed them. This is very satisfactory as far as it goes. But we may deduce a precisely similar conclusion in a much simpler manner, from considering the form and distribution of the lines of magnetic force in the field. These are shewn in Fig. 7, together with the exploring coil situated as in Fig. 4. A simple inspection of the figure will show that at 0° a certain number of lines of force would thread themselves through the exploring coil. As the coil moved around toward the S pole, the number would increase at first, then become for an instant stationary, with neither increase nor decrease; after that a very rapid decrease would set in, which, as the coil passed the 90° point, would result in there being no lines of force through the coil. But at the very same instant the lines of force would begin to crowd in on the other side of the coil, and the number so threaded through negatively would increase until the coil turned around to about the position marked T, where the lines of force are nearly tangential to its path and here the inversion would occur, because, from that point onwards to 180°, the number of lines of force threaded through the coil would decrease. We see, then, that such inversions in the induction must occur of necessity to a small coil rotating in a magnetic field in which the lines of force are distributed in the curved directions, and with the unequal density which this disposition of the field magnets presents. The remedy is obvious; arrange a more uniform field in which the lines of force are more equally distributed, and are straighter.

Fig. 7

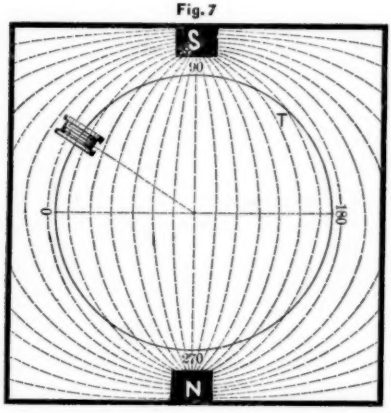
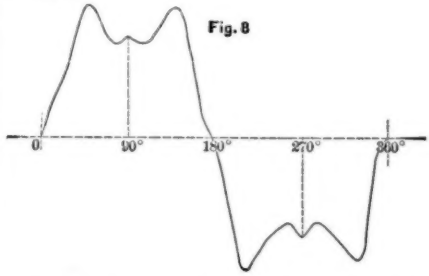
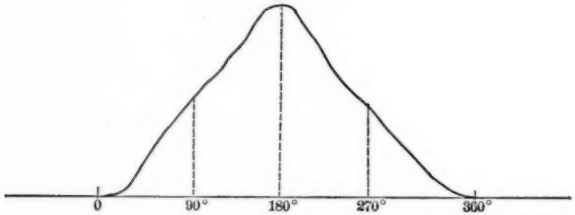


Fig. 8



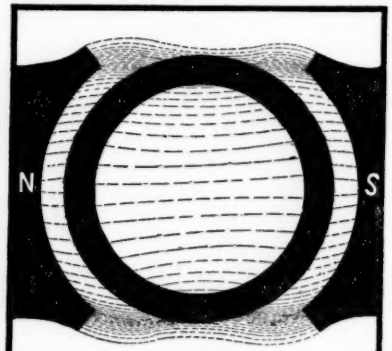
If an iron core be substituted for the wooden core the useful induction is greater and the false induction less; there is still an inversion, but it takes place at about 25° from the pole, and is quite trifling in amount. The introduction of iron pole pieces extending in two nearly semi-circular arcs from the magnets on either side has, if the wooden ring be still kept as a core, the effect of completely changing the induction, so that the curve, instead of showing a maximum at 90° from starting, shows one at about 10°, and another at 170°. If, however, we make the double improvement of using the iron pole pieces and the iron core at the same time, the effect is at once changed. There are no longer any inversions, though the induction shows some peculiarity still. Fig. 8

Fig. 9



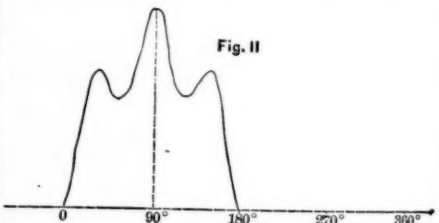
shows the curve of induction adapted from Dr. Isenbeck's paper, and Fig. 9 the curve of potential, which I have had integrated from it. Looking at Fig. 8 we see that on starting from 0° induction soon mounts up, and becomes a maximum at about 20°, where the coil is getting well opposite the end of the encircling pole piece. From this point on, though the induction is somewhat less, it still has a high value, showing a slight momentary increase as the coil passes the pole at 90°, and there is another maximum at about 160°, as the coil passes the other end of the pole piece. My integrated curve (Fig. 9) tells us what would go on at the collector if this were the action in the connected set of coils of a Pacinotti or Gramme ring. The potential rises from 0° all the way to close upon 180°. Still this is not perfect. In the perfect case the potential

Fig. 10



curve would rise in a perfect harmonic wave form, like that shown in Fig. 2. Fig. 9 departs widely from this, for it is convex from 0° to 90°, and concave between 90° to 180°. But there are no inversions. The cause of the improvement is easily told: The field—such as there is between the pole piece and the core—is "straighter," and the density of the lines of force in it more uniform. I proved this experimentally in 1878, by the simple process of examining the lines of force in such a field by means of iron filings; the actual filings, secured in their places upon a sheet of gummed glass, were sent to the late Mons. Alfred Niaudet, who had requested me to examine the matter for him. Fig. 10 shows the actual field between the encircling pole pieces and the iron ring. It will be seen that, though nearly straight in the narrow intervening region, they are not equally distributed, being slightly denser opposite the ends of the pole pieces. One other case examined by Dr. Isenbeck, we

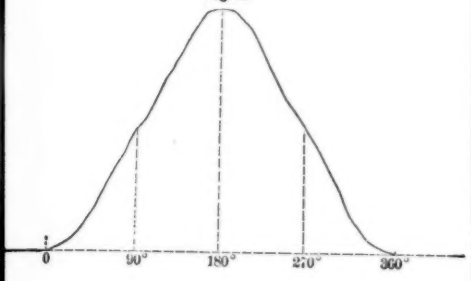
Fig. 11



will glance at. The effect of introducing within the ring an interior magnet, having its S pole opposite the external S pole, and its N pole opposite the external N pole, was found to assist the action. The induction curve is represented in Fig. 11. As will be seen, there are two maxima at points a little beyond the ends

of the pole pieces, as before; but in between them there is still a higher maximum, right between the poles. This case also has been integrated on Mr. Boys' machine, and shows the potential curve of Fig. 12. This curve is a still nearer approach to the harmonic wave form, being concave from 0° to 90°, and convex

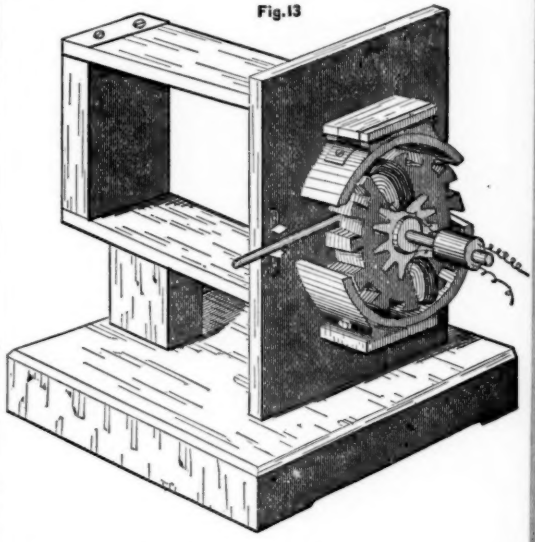
Fig. 12



I pass from Dr. Isenbeck's researches, and the integrated curves of potential which I have deduced from them, to some further researches of my own, which were undertaken with the view of throwing some light on the question whether the Pacinotti form of armature, with protruding iron teeth, or the Gramme form, in which the iron core is entirely overwound with wire, is the better. It has been assumed without, so far as I am aware, any reason assigned, that the Gramme ring was an improvement on that of Pacinotti. Pacinotti's was of solid iron, with teeth which projected both outwards and inwards, having the coils wound between. Gramme's was made "either out of one piece of iron, or of a bundle of iron wires," and had the coils wound "around the entire surface." Now the question whether the Gramme construction is better than the Pacinotti or not, can readily be tested by experiment. And experiment alone can determine whether it is better to keep a thickness of wire always between pole pieces and the core, or to intensify the field by giving to the lines of force the powerful reinforcement of protruding teeth of iron. The apparatus I have constructed for determining this point is now before you. It is sketched in Fig. 13.

First there are a couple of magnets set in a frame so as to give us a magnetic field, and there are pole pieces that can be removed at will; in fact, there are three sets of pole pieces for experimenting with different forms. Between the

Fig. 13



poles is set an axis of brass, upon which the armatures can be slid. These armatures are three in number. One is shown in Figs. 14 and 15, and consists of two coils of fine wire wound upon a wooden ring; another armature is exactly like

Fig. 14

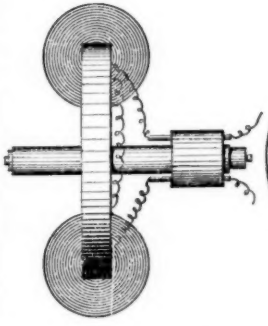
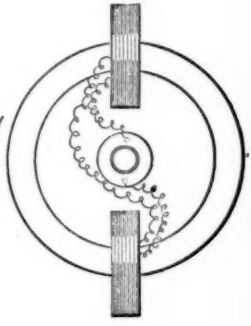


Fig. 15



this, but is built up on a ring of iron wire; a third (shown in its place in Fig. 13) is constructed upon a toothed ring made up of a number of plates of ferrotype iron, cut out and placed flat upon one

another. On each of the armatures are wound two coils at opposite ends of a diameter. The coils contain precisely equal lengths of silk-covered copper wire, cut from one piece. The cross section of the core within each of these coils is in each case a square, of one centimeter in the side, so that the number of turns in each coil is as nearly equal as possible. I can slip any one of these armatures into the field, and connect it with a galvanometer. There is a lever handle screwed to the armature, by means of which it can be moved. I have used two methods of proceeding in order to compare the coils. One of these methods is to turn the armature suddenly through a quarter of a revolution, so that the coils advance from 0° to 90°, when the "throw" of the needle of the galvanometer—which is a slow beat one—gives me a measure of the total amount of induction in the armature. The results are as follows:

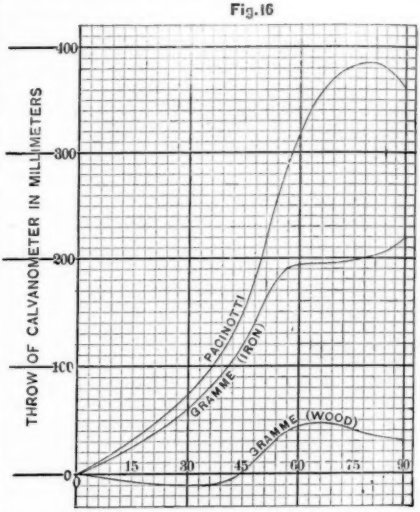
Gramme. Wooden Ring.	Gramme. Iron Ring.	Pacinotti. Iron Toothed. Ring.
5	24	50

My second method of using these armatures consists in jerking the coils through a distance equal to their own thickness, the coils being successively placed at different positions in the field, the throw of the galvanometer being observed as before. Each of the coils occupies as nearly as possible 15° of angular breadth. Accordingly I have two stops set, limiting the motion of the handle to that amount, and at the back there is a graduated circle enabling me to set the armature with the coils in any desired position. If we move the coils by six such jerks, through their own angular breadth each time, then starting at 0°, the sixth jerk will bring us to 90°. I have plotted out in Fig. 16 the three curves thus obtained, and the corresponding numbers are given in the following table:

	Gramme. Wooden Ring.	Gramme. Iron Ring.	Pacinotti. Iron Tooth'd Ring.
0°–15°	5	25	30
15°–30°	10	60	70
30°–45°	0	120	140
45°–60°	45	195	320
60°–75°	40	200	380
75°–90°	30	220	360

These figures leave no doubt as to the question at issue. The Gramme pattern of ring armature, so far from being an improvement on the Pacinotti, is distinctly a retrograde step; always supposing that the cost of construction, liability to heating, and other kindred matters be equal for the two. The significance of this point will be resumed at a later period in this paper.

Fig. 16



Before leaving the theory of armatures to pass to that of field magnets, I should wish to say that the experiments which I have made, and also those of Dr. Isenbeck, have been so instructive to myself that I have already begun a similar series of observations on other forms of armature. I hope in due time to make known the results of my investigations.

But little advance has been made in theory so far as relates to field magnets. The law of saturation of an electro-magnet remains still an empirical law. It is satisfactory, however, that such widely differing authorities as Prof. Clausius, M. Marcel Deprez, and Professors Ayrton and Perry, agree in accepting the empirical formula of Frölich as a sufficiently accurate expression for the law of saturation.

Some progress has been made in the theory of the lead that must be given to the brushes of the dynamo. Formerly this was ascribed to a sluggishness in

the demagnetization of the iron of the armature; but in 1878 the late M. Antoine Breguet suggested as a reason the influence of the actual current circulating around the armature coils, which would tend to produce in the iron of the armature a magnetization at right angles to that due to the field magnets.

Breguet showed that there would be a resultant oblique direction of the lines of magnetization in the field, and therefore, since the "diameter of commutation" is at right angles to this direction, the brushes also must be displaced through an equal angle. Clausius accepts this view in his recent theory, and adopts for the angle of the resultant field that whose tangent is the ratio of the two magnetizing forces due to the field magnets and the current in the armature respectively. Professors Ayrton and Perry have also pointed out that there will be an additional displacement of the resultant poles of the armature, consequent upon the self-induction going on in the armature coil between its different sections. In their paper on the government of motors, in which they have brought out this point, they, however, take the view that part of the displacement of the pole is due to the sluggishness of demagnetization of the iron. I do not think, however, that this can be maintained. No experimental proof has ever been given that there is any such thing as a true magnetic lag; the apparent magnetic sluggishness of thick masses of iron is demonstrably due to internal induced currents; and no one uses solid iron in armature cores for this very reason. Neither has it been shown that thin iron plates or wires, such as are used in armature cores, are slower in demagnetizing than magnetizing. Indeed, the reverse is probably true; and, until further experimental evidence is forthcoming, I shall assume that there is no magnetic lag in properly laminated iron cores.

It may here be pointed out that, assuming as a first approximation that the rule that the tangent of the angle of lead represents the ratio between the magnetizing power of the field magnets and of the armature coils, the lead may be diminished to a very small quantity, by increasing the relative power of the field magnets, a course which is for many other reasons advisable. All practice confirms the rule that the magnetic moment of the field magnets ought to be very great as compared with that of the armature. Further than this there ought to be so much iron in the armature as to be just saturated when the dynamo is working at its greatest activity. If there is less than this it will become saturated at a certain point, and when any currents greater than this are employed, the lead will alter, for then the magnetic effect due to the current in the armature will be of greater importance relatively to that due to the field magnets. For the same reason the lead will be more constant when the field magnets are under their saturation point than when quite saturated. In short, every cause that tends to reduce the lead makes the lead more constant, and therefore tends to reduce sparking at the brushes. And the best means to secure this is obviously to use an unstinted quantity of iron—and that of the softest kind—both in the field magnets and in the armature, for then the currents circulating in the armature will have less chance of perturbing the field.

In relation to the magnetism of field magnets, it may be pointed out that the "characteristic" curves now so much used for the study of the action of dynamo machines, which show the rise of the electromotive force of the machine in relation to the corresponding strength of the current, are sometimes assumed, though not quite rightly, to represent the rise of magnetization of the field magnets. Now, though the magnetization of the magnet may attain to practical saturation, it does not, under a still more powerful current, show a magnetization less than saturation. But the characteristics of nearly all series-wound dynamos show—at least for high speeds—a decided tendency to turn down after attaining a maximum; and for some machines, for example, the Brush, this diminution of the electromotive force is very marked. The electromotive force diminishes, but the magnetism of the field magnets does not. An explanation of this dip in the characteristic has lately been put forward by Dr. Hopkinson in his lecture on "Electric Lighting," before the Institution of Civil Engineers, attributing this to the reaction of self-induction and mutual induction between the sec-

tions of the armature. No doubt this cause contributes to the effect, as all such reactions diminish the effective electromotive force. I am inclined, however, to think that the greater part of the effect is due to the shifting of the effective line of the field in consequence of the iron of the field magnets becoming saturated before the armature is so. It is at least significant that in the Brush machine, where the reduction of the electromotive force is very great, there is also such a mass of iron in the armature, and so variable a lead at the brushes.

THE COMPOUND STEAM ENGINE, CONSIDERED IN CONNECTION WITH ITS STEAM-GENERATING PLANT.

By RICHARD H. BUEL, C. E.

Contributed to VAN NOSTRAND'S ENGINEERING MAGAZINE.

THE compound, or "double-cylinder" steam engine, as it is often called in distinction to the non-compound or "single-cylinder" engine, has practically supplanted the latter for marine purposes, is largely used in factories, and has recently been applied to locomotives.

Simultaneously with the introduction of double-cylinder engines, the steam pressure carried in marine boilers was increased, and in general, the ratio of expansion also; so that there were many engineers who were inclined to attribute the economy of the new system, which was indubitable, to these latter changes rather than to the compounding. Indeed, there are many engineers to-day, who hold this opinion,—and although the disputes in relation to the matter, which have for several years occupied so much space in technical journals, are less animated than they formerly were, they have not wholly ceased. The greater number of these discussions have been theoretical rather than practical, and experiments, when they were presented, have frequently proved to be of little value, on account of differences in important conditions. Many experimental results that are valuable have, however, been published; and the writer proposes, in the present article, to give a brief summary of results that seem be of practical importance and interest, relating, as they do, to the various means that have been employed to secure economy in the production of steam power.

The economy of a steam engine, considered as a whole, depends upon the cost of producing the steam in the boilers, and the efficiency of the motor in which this steam is used. Thus, if one steam engine produces an indicated horse-power with 23 lbs. of steam per hour, and this steam is furnished by a boiler which evaporates 8 lbs. of water per lb. of coal,–while another engine requires 25 lbs. of steam per hour for each indicated horse-power developed, and obtains its steam from a boiler which evaporates 9 lbs. of water per lb. of coal, the consumption of coal per hour for each indicated horse-power developed will be, for the first engine, 2.875 lbs. and for the second, 2.778 lbs. In order to make a perfectly accurate comparison of the cost of coal or steam per horse-power, the useful power exerted, or the indicated power less the power absorbed by friction of mechanism, should be used as the basis of calculation. As a general rule, however, the useful or net horse-power is not determined, in making experiments, so that it is necessary to refer the expenditure of steam and coal to the indicated horse-power, with the assumption, which is not far from the truth, that the pressure required to overcome the friction of well designed steam engines is practically constant.

In comparing experiments made with different engines, or with the same engine under different circumstances, if the steam pressures vary in the several experiments, as is generally the case, it is more accurate to reduce the weights of steam used, to equivalent weights at the pressure of the atmosphere. This is particularly useful, when the engines are considered in connection with the boilers, since it is customary to compare the performance of different boilers, with respect to their "equivalent" evaporation, or evaporation from and at 212°, that is to say, the evaporation at atmospheric pressure of water whose temperature is 212°. In the ex-

TABLE I.

FACTORS FOR REDUCING EVAPORATION FROM DIFFERENT TEMPERATURES AND AT DIFFERENT PRESSURES, TO EVAPORATION FROM AND AT 212°.

Temperature of Feed Water.	Pressure of Steam in lbs. per Square Inch above the Atmosphere.										
	0	10	20	30	40	50	60	70	80	90	100
32°	1.187	1.195	1.201	1.206	1.210	1.214	1.217	1.220	1.222	1.224	1.226
40°	1.179	1.187	1.193	1.198	1.202	1.206	1.209	1.212	1.214	1.216	1.218
50°	1.168	1.176	1.182	1.187	1.191	1.195	1.198	1.201	1.203	1.205	1.207
60°	1.158	1.166	1.172	1.177	1.181	1.185	1.188	1.191	1.193	1.195	1.197
70°	1.148	1.156	1.162	1.167	1.171	1.175	1.178	1.181	1.183	1.185	1.187
80°	1.137	1.145	1.151	1.156	1.160	1.164	1.167	1.170	1.172	1.174	1.176
90°	1.127	1.135	1.141	1.146	1.150	1.154	1.157	1 160	1.162	1.164	1.166
100°	1.117	1.125	1.131	1.136	1.140	1.144	1.147	1.150	1.152	1.154	1.156
110°	1.106	1.114	1.120	1.125	1.129	1.133	1.136	1.139	1.141	1.143	1.145
120°	1.096	1.104	1.110	1.115	1.119	1.123	1.126	1.129	1.131	1.133	1.135
130°	1.085	1.093	1.099	1.104	1.108	1.112	1.115	1.118	1.120	1.122	1.124
140°	1.075	1.083	1.089	1.094	1.098	1.102	1.105	1.108	1.110	1.112	1.114
150°	1.065	1.073	1.079	1.084	1.088	1.092	1.095	1.098	1.100	1.102	1.104
160°	1.054	1.062	1.068	1.073	1.077	1.081	1.084	1.087	1.089	1.091	1.093
170°	1.044	1.052	1.058	1.063	1.067	1.071	1.074	1.077	1.079	1.081	1.083
180°	1.033	1.041	1.047	1.052	1.056	1.060	1.063	1.066	1.068	1.070	1.072
190°	1.023	1.031	1.037	1.042	1.046	1.050	1.053	1.056	1.058	1.060	1.062
200°	1.013	1.021	1.027	1.032	1.036	1.040	1.043	1.046	1.048	1.050	1.052
212°	1.000	1.008	1.014	1.019	1.023	1.027	1.030	1.033	1.035	1.037	1.039

amples of engine performance that follow, this reduction has been made, on the assumption that the temperature of the feed water was 32°, and using the pressure noted in each experiment. Such reductions are facilitated by the factors of Table I. For the sake of illustration suppose, in the case of experiment 1, Table IV. that the result there given is to be reduced to the equivalent evaporation of water which is actually evaporated from a temperature of 150° and at a pressure of 67.6 lbs. sq. in. above the atmosphere. By Table I. it appears that the equivalent evaporation, under these circumstaces, is

$$\frac{1.097}{1.219} \times 22.4 = 20.2 \text{ lbs.}$$

In discussing the cost of the production of steam, and its use in an engine, it will be convenient to treat these two topics separately.

I. THE ENGINE.

Experiment and theory both show clearly that in order to use steam economically in an engine, it must be allowed to expand in the cylinder. Precisely what ratio of expansion it is most economical to adopt for any special case may not be so well settled; but this much seems to be clearly established: that such ratios of expansion as are known to be economical cannot be used without serious inconvenience in engines which, as is the case with those of the marine type, are not provided with fly-wheels. This is because the pressure on the crank-pin of an engine in which steam is used with a high ratio of expansion, varies so much that the frame and other parts of the engine are unduly strained. The effect of a fly-wheel in preventing serious injury to an engine subjected to varying strains, and the effect of these strains when they are transmitted directly to the machine, can be most simply illustrated by experiment. The following test was made by the writer: A vertical steam engine, with plain slide-valve admitting steam for the full stroke, was used to drive a train of wire rolls in a steel mill. The speed of the engine was controlled by a centrifugal governor which acted on a throttle-valve, and on the engine shaft there was an enormous fly-wheel, 23 ft. in diameter and weighing 27.5 gross tons. The maximum power of this engine—436 horse-power—was exerted when 8 simultaneous passes were made in the rolls (6 in the wire rolls and 2 in the roughing

rolls), and this power was instantly reduced to 36 horse-power when the rolls were running idle. Under these circumstances, there was no perceptible change in the speed of revolution of the engine, and it had been used in work of this kind for a number of years, at a very slight expense for repairs. In the same mill, connected to another train of rolls, where the work was not so hard, was an automatic cut-off engine, of much greater power than the vertical engine, but with a very light fly-wheel,—and after this engine had come to a stop several times, when two simultaneous passes were made in the rolls, the fly-wheel was taken off and a heavier one was substituted, which change put an end to the trouble.

A striking example of the effect of expansion in an engine which had no fly-wheel, is contained in Chief-Engineer J. W. King's "Report on European Ships of War, etc.," Washington, 1877, as follows:

"The most expensive and notable attempt to realize the benefits of the compound system by the simple engine at sea was made two years ago by the proprietors of the Allen Line of steamers. This company made the comparative test on a large scale. Two ships were built, the one fitted with compound engines, and the other with simple expansive engines. The boilers were identically alike, made from the same drawings, having the same grate and heating surface, and the same pressure of steam was used in each vessel.

"I have not at command all the details of these ships, but I received from the designer of the machinery the following particulars:

	Name of ship, Polynesia.	Name of ship, Circassian.
Length, feet.....	400	360
Breadth, "	42	40
Draught of water, ft.	25.5	23
Kind of engines....	Compound.	Non-Comp'nd.
Number of cylinders	4	2
Diameter, inches...	43 and 80¾	62
Stroke, feet.....	4	4
Number of boilers..	10	10
Number of furnaces	20	20
Pres're of steam, lbs.	60	60

"The expansion valves were fitted in the simple engines to work the steam, when desired, to the same degree of expansion as in the compound engines. The workmanship and materials were equally good, and the parts equally strong in each set of engines. The two ships were put on the line between Liverpool and Quebec, Canada, and, as was anticipated, the results as to economy of fuel were not materially different, about two pounds of good Welsh coal per indicated horse-power per hour being expended in each ship. This satisfactory result, however, soon found an offset in the shape of unexpected difficulties with the simple engine, consequent upon the serious shocks resulting from the rapidly varying pressures on the crank-pins. So serious were these, that not only the crank-shaft, but also the stationary parts of the engines, began at an early day to show signs of weakness, and in a short time gave out altogether. The superintending engineer of the company was the designer of the machinery, and it was only after his skill and efforts failed to keep the ship running that he reluctantly decided to remove the engines and to substitute compound engines in their stead. The engines substituted had a pair of vertical inverted cylinders, with a diameter for the high pressure of 55 inches, and for the low pressure of 92 inches.

"The performance of the Polynesia was satisfactory from the first, the voyages never having been interrupted; and the performance of the Circassian has also been satisfactory since the substitution in her of the compound engines for the simple ones."

It frequently happens that a wasteful single-cylinder steam engine can be made much more economical by changing it to a compound engine. The principal reason for the economy that results from such a change seems to be the suppression of a large part of the cylinder condensation. The cylinder of an engine is exposed, during each stroke, to the extremes of temperature due to the entering steam and the exhaust. Now, if the steam is used in a double-cylinder engine, the extremes of temperature in the small cylinder are inconsiderable, and in consequence the condensation in this cylinder in usually slight in amount, and

the extremes of temperature in the large cylinder are also less than they would be if the small cylinder were removed.

Tables II. and III. contain the results of some experiments made by the writer with small engines, both before and after

TABLE II.

TEST OF SMALL ENGINE BEFORE AND AFTER ADDITION OF SECOND CYLINDER—CYLINDERS NOT JACKETED—CUT-OFF ALIKE IN BOTH CYLINDERS—VOLUME OF LARGE CYLINDER =2×VOLUME OF SMALL CYLINDER.

Item.	1	2	3	4	5
	Single Cylinder.		Compound.		
	Cut-off, $\frac{1}{4}$.	Cut-off, $\frac{1}{6}$.	Draining water from steam before admission into 2d cylinder.		Not draining water from steam
			Cut-off, $\frac{1}{2}$.	Cut-off, $\frac{2}{3}$.	Cut-off, $\frac{1}{2}$.
Initial pressure of steam, lbs. per sq. in. above atmosphere	90	99	106	100	97
Lbs. of water evaporated per hour from and at 212°, per indicated horse-power.....	47.1	51.9	29.5	33.2	34.8

TABLE III.

EXPERIMENTS ON SMALL ENGINE, SINGLE CYLINDER AND COMPOUND, LARGE CYLINDER STEAM JACKETED, SUPERHEATER FOR STEAM DISCHARGED FROM SMALL CYLINDER, AND FEED WATER HEATER—CUT-OFF ALIKE IN BOTH CYLINDERS—VOLUME OF LARGE CYLINDER=2×VOLUME OF SMALL CYLINDER.

Item.	1	2	3	4	5	6	7
	Single cylinder, condensing.		Compound, condensing.				Compound Non Condensing
	Steam jacket in use.	Steam jacket not in use.	Steam jacket, superheater and heater in use.		Steam jacket and superheat'r in use. Heater not in use.	Steam jacket, superheat'r and heater not in use.	Steam jacket, superheat'r and heater in use.
	Cut-off, $\frac{1}{4}$	Cut-off, $\frac{1}{4}$	Cut-off, $\frac{2}{3}$	Cut-off, $\frac{1}{2}$	Cut-off, $\frac{1}{2}$	Cut-off, $\frac{1}{2}$	Cut-off, $\frac{1}{2}$
Initial pressure of steam, lbs. per sq. in. above atmosphere	89.8	90.4	84.7	90.3	93.5	95.3	90.7
Lbs. of water evaporated per hour from and at 212°, per indicated horse-power.....	34.8	41.1	29.9	29.	29.3	30.4	35.6
Water per hour not accounted for by diagrams from small cylinder.....	—	—	76	26	43	22	28
Water per hour not accounted for by diagrams from large cylinder.....	157	341	368	141	143	304	95

compounding. These experiments show, in addition, the gain produced by the means usually employed to prevent cylinder condensation, viz., superheating and steam jacketing. It will be seen that the effect of the various changes made in the engines is to furnish examples of nearly every form of single and double-cylinder engines in use to-day, with the exception of double-cylinder engines having both cylinders jacketed, illustrations of which are given in subsequent tables. These examples may be classed as follows:

Table.	Experiment.	
II.	1, 2	Single cylinder without jacket.
III.	2	Single cylinder without jacket.
III.	1	Single cylinder, steam jacketed.
II.	3, 4	Double cylinder, with receiver, large cylinder not jacketed.
II.	5	Double cylinder, without receiver, large cylinder not jacketed.
III.	6	Double cylinder, with receiver, large cylinder not jacketed.
III.	3, 4, 5, 7	Double cylinder, with receiver, large cylinder steam jacketed.

The results of some experiments made with engines of United States Revenue steamers are contained in Table IV. These experiments are selected from a large number published in the "Transactions of the American Society of Civil Engineers," III., 368, and in the "Report of the Trial of the Steam Machinery of the United States Revenue Steamer Gallatin," Washington 1875.

It will be seen that the several experiments of each set are not in all cases strictly comparable, owing to variations in steam pressure and ratio of expansion, although the results here given have been selected with care from all the experiments, as being the most available for comparison. It is probable, however, that these experiments, taken as a whole, represent about the best, as well as the average performance of single and double-cylinder marine engines using saturated steam.

It has been stated that one of the means employed for preventing cylinder condensation is to superheat the steam before its admission into the cylinder of the engine; and the results obtained with a single-cylinder engine, not jacketed, and given in Table V., show that this method is very efficient. In these experiments, which were made in 1862, the amount of water evaporated by the boilers was not measured. After the completion of the experiments with saturated steam, however, and before putting in the superheaters, the evaporative performance of the boilers with anthracite coal was determined, so that an estimate can be made, with considerable accuracy, of the evap-

TABLE IV.

EXPERIMENTS ON SINGLE AND COMPOUND ENGINES OF UNITED STATES REVENUE STEAMERS.

Item.	1	2	3	4	5	6	7	8	9
	Rush.	Bache	Bache	Dext'r	Dallas	Gallatin.			
	Compound, condensing, with receiver. Both cylinders steam jacketed.	Compound, condensing, with receiver. Large cylinder steam jacketed.	Single, condensing, cylinder jacketed.	Single, condensing, cylinder not jacketed.	Single, condensing, cylinder not jacketed.	Single, condensing.		Single, condensing without vacuum.	
						Steam jacket not in use.	Steam jacket in use.	Steam jacket not in use.	Steam jacket in use.
Initial pressure of steam, lbs. per sq. in. above atmosphere	67.6	74.1	76.0	65.7	32.2	39.2	40.0	41.6	64.1
Ratio of expansion.....	6.22	5.1	5.11	4.46	5.07	5.07	4.49	5.92	3.52
Lbs. of water evaporated per hour from and at 212°, per indicated horse power....	22.4	27.3	28.3	29.2	32.3	29.0	30.0	31.5	33.0

TABLE V.

EXPERIMENTS WITH SATURATED AND SUPERHEATED STEAM IN ENGINE OF STEAMER GEORGEANNA.

Item.	1	2	3	4	5	6	7
	Saturated steam.		Superheated steam.				
	Cumberland coal.		Cumberland coal.		Anthracite coal.		
Initial pressure of steam, lbs. per sq. in. above atmosphere.	23.1	17.2	22.5	28.2	31.3	22.7	16.4
Cut-off	0.45	0.65	0.45	0.65	0.28	0.45	0.65
Indicated horse-power	429	393	466	453	456	451	433
Coal per indicated horse-power per hour	3.38	3.71	2.91	2.99	2.43	2.52	2.72
Temperature of steam	265°	253°	344°	338°	336°	336°	329°
Probable evaporation per hour from and at 212°, per indicated horse-power, lbs.	—	—	—	—	23.45	24.41	26.25

TABLE VI.

EXPERIMENTS WIEH SATURATED AND SUPERHEATED STEAM IN SMALL NON-CONDENSING ENGINE.

Item.	1	2	3	4	5	6
	Saturated steam.			Superheated steam.		
Initial pressure of steam, lbs. per sq. in. above atmosphere.	50.4	50.2	50.3	50.4	50.0	50.2
Cut-off	0.247	0.465	0.701	0.248	0.461	0.685
Temperature of steam in steam pipe near throttle	302°	303°	303°	478°	441°	406°
Temperature of steam in cylinder	278° to 297°	279° to 296°	282° to 300°	313°	316°	315°
Temperature of steam in exhaust pipe	210°	210°	213°	217°	210°	212°

oration in the engine experiments when anthracite coal was used. With regard to the temperatures given in Table V., it is stated in the original report of the trials that they are all probably too low, but it is probable also that the temperatures of the saturated and superheated steam all differ from the true figures by an amount which is sensibly constant. Objections have frequently been made to the use of superheated steam in the cylinders of engines, on the ground that it is injurious to the rubbing surfaces, since it prevents thorough lubrication. This objection is undoubtedly well founded, if the superheating is excessive, but when it is just sufficient to prevent condensation, the foregoing objection loses its force. This is illustrated in Table VI., which contains the results of some experiments made with the apparatus designed by Mr. G. B. Dixwell—consisting of a superheater so arranged that superheated steam could be mixed with saturated steam before its admission into the cylinder of the engine, to produce a mixture of any desired temperature—and a sensitive cylinder pyrometer by means of which the changes of temperature in the cylinder during each stroke could easily be determined. These experiments show that the amount of superheating necessary to prevent cylinder condensation is different for each ratio of expansion, and that if the required temperature is not exceeded, the resultant cylinder tempera-

tures are by no means excessive. Many instances are known to the writer of the benefits of superheating steam before admitting it into the cylinder of an engine, the effect of superheating being ordinarily to render the engine more economical and more powerful at the same time.

The double-cylinder engine is largely used on land, particularly in the case of engines employed for pumping water for public supply. The designer of a land engine is not limited to the space prescribed for the engine of a vessel, and there are several considerations which render it possible to make a land engine, in general, more economical than one of the marine type. Table VII. contains but few examples, but these have been chosen from a large number, with the idea of giving the extremes and average of the economy obtained from the most prominent types of double-cylinder land engines.

The modern forms of stationary steam engines, commonly known as automatic cut-off engines, are ordinarily single-cylinder engines, of excellent construction, and their performance is exceedingly economical, more so, in fact, than that of the average double-cylinder engine. The results obtained with such engines seem to be due to the high piston-speed, the small fraction of clearance, and the carefully designed valve-gear; the effect of these conditions being to maintain the cylinder pressure nearly equal to that in the boiler, until the point of cut-off, when the admission valve is quickly and tightly closed–while the high piston-speed makes the cylinder condensation a comparatively small fraction of the total steam consumption. A few examples of the performance of automatic cut-off engines, single-cylinder, with and without steam jackets, are shown in Tables VIII. and IX.

The examples that have been given in the preceding tables, although comparatively few in number, are believed to be representative ones. It becomes, therefore, an interesting question, to determine the commercial economy of these different forms of engines, when using steam furnished by boilers of varying evaporative efficiency.

In order to do this, the most economical and the most wasteful examples of each type of condensing engine have been selected, and the cost in coal and combustible of the steam used per hour for each indicated horse-power developed has been determined, with data given in the second part of the present article, for the most economical and the most wasteful boiler performance there illustrated, excluding the experiments in Table XXVI, where the conditions were abnormal. The results of this comparison are contained in Table X.

TABLE VII.

TESTS OF COMPOUND PUMPING ENGINES.

Item.	1	2	3	4	5
	Holly, Buffalo, no receiver, Cylinders steam jacketed.	Leavitt, Lawrence, no receiver Cylinders steam jacketed.	Warden, Cincinnati, receiver, Cylinders not jacketed.		Worthington, Roxbury, no receiver, Cylinders not jacketed.
			1st trial.	2d trial.	
Initial pressure of steam, lbs. per sq. in. above atmosphere.....	67.9	89.5	125.	125.	39.
Ratio of expansion.....	6.9	16.4	3.33	3.38	3.1
Lbs. of water evaporated per hr. from and at 212°, per indicated horse power..... ...	26.2	20.1	39.2	39.1	35.7

TABLE VIII.

TESTS OF SINGLE CYLINDER STEAM ENGINES AUTOMATIC CUT-OFF, CONDENSING. CYLINDERS NOT JACKETED.

Item.	1	2	3	4
	Engines at Miller's International Exhibition, Cincinnati, 1880.			Harris-Corliss Engine at Works of National RubberCo.
	Reynolds-Corliss.	Harris-Corliss.	Wheelock.	
Initial pressure of steam, lbs. per sq. in. above atmosphere	92.5	91.6	91.4	70.1
Ratio of expansion	6.67	6.95	6.35	9.72
Lbs. of water evaporated per hour from and at 212° per indicated horse-power	23.9	23.7	23.6	23.3

TABLE IX.

TEST OF CORLISS STEAM ENGINE, CONDENSING, WITH AND WITHOUT STEAM-JACKET, AT MULHOUSE.

Item.	1	2	3	4
	Steam-jackets not in use.	Steam-jacket of cylinder in use.	Steam-jackets of cylinder and piston in use.	
Initial pressure of steam, lbs. per sq. in. above atmosphere	66.2	68.1	67.9	67.2
Ratio of expansion	7.9	10.83	10.83	7.9
Lbs. of water evaporated per hour from and at 212° per indicated horse-power	30.45	23.11	22.56	22.59

TABLE X.

POUNDS OF ANTHRACITE COAL AND COMBUSTIBLE PER HOUR PER INDICATED HORSE-POWER, USED BY DIFFERENT TYPES OF CONDENSING ENGINES, OBTAINING STEAM FROM BOILERS OF DIFFERENT ECONOMICAL EFFICIENCY.

Type of Engine.	Reference Number.		Pounds of coal per hour, per indicated horse-power.		Pounds of combustible per hour, per indicated H. P.	
	Table.	Experiment.	Boiler, Table XXV, Experim't 5.	Boiler, Table XXI, Experim't 9.	Boiler, Table XXV, Experim't 5.	Boiler G, Table XV.
Double cylinder, steam jacketed.	VII	2	1.71	3.00	1.35	2.39
	III	3	2.55	4.47	2.00	3.56
Double cylinder, not jacketed	II	1	2.51	4.40	1.97	3.51
	VII	2	3.33	5.85	2.63	4.66
Single cylinder, steam jacketed.	IX	3	1.92	3.37	1.51	2 68
	III	1	2.96	5.20	2.33	4.14
Single cylinder, not jacketed	VIII	4	1.98	3.48	1.56	2.77
	II	2	4.41	7.75	3.48	6 18
Single cylinder, not jacketed, superheated steam	V	5	1.99	3.50	1.57	2.79
	V	7	2.23	3.92	1.76	3.12

REPORTS OF ENGINEERING SOCIETIES.

AMERICAN SOCIETY OF CIVIL ENGINEERS, April 2d, 1884.—The Society met at 8 P.M., Vice President Wm. H. Paine in the Chair, John Bogart, Secretary.

Ballots were canvassed and the following candidates elected:—

As Members:—Thomas W. Baldwin, Bangor, Me.; Thomas E. Brown, Jr., New York City; Oren B. Colton, Chicago, Ill.; Stewart Derbishvie, Aylmer, Canada; Joshua L. Gillespie, St. Paul, Minn.; Minard L. Holman, St. Louis, Mo.; William T. Jennings, Toronto, Canada; Henry F. Juengst, St. Joseph, Mo.; Moritz Lassig, Chicago, Ill.; John F. O'Rourke, New York City; Geo. W. Rafter, Fredonia, N. Y.; Irving A. Stearns, Wilkesbaire, Pa.

As Juniors:—Frank E. Bissel, Sedalia, Mo.; Joseph A. Powers, Landsingburg, N. Y.; Commodore P. Ruple, Wilson's Point, La; William H. Starr, Buffalo, N. Y.

A paper by the late Wm. R. Morley, M. Am. Soc. C. E. on the Proper Compensation for Railroad Curves, which had been read at a previous meeting of the Society, was discussed.

Mr. A. A. Robinson, M. Am. Soc. C. E., stated that the location of the New Mexican Extension of the Atchison, Topeka and Santa Fe Railroad was made with the standard of compensation, based upon the theory that each degree of curvature was the equivalent in resistance to the movement of trains of $\frac{5}{100}$ of a foot of ascent. After the construction it was found: first, that upon maximum gradients 0.6 per cent. where a full train was 30 to 32 loaded cars, this compensation was hardly sufficient; second, upon maximum gradients $1\frac{13}{100}$ per cent. with full train of 18 to 20 cars, it was fully sufficient; third, upon maximum gradient of 3.4 per cent. with full train of 7 to 8 cars it was evidently greater than was needed. Mr. Robinson considers that the resistance due to curvature is affected by so many conditions that it cannot be determined by mathematical formula except for the particular conditions assumed in a special case. The speed of the train, the elevation of rail, the greater length of outer rail, the gauge of track as compared with that of the wheels, all affect the question, and the resultant of all the forces will be a function not only of the rate of curvature and of the gross tonnage of the train, but also of the number of cars in the train. A train of 10 cars will produce a resistance greater than 10 times that of a single car. Upon a division of the railroad where locomotives can pull 30 cars, the rate of compensation should be greater than upon a division where engines can pull but 10 cars. In practice Mr. Robinson has adopted the following rules for compensation:

Rate of maximum grade 0.0 to 0.6 per 100,
Compensation 0.06 per 100.
Rate of maximum grade 0.6 to 1.6 per 100,
Compensation 0.05 per 100.
Rate of maximum grade 1.6 to 3.0 per 100,
Compensation 0.04 per 100.

Mr. Wm. H. Searles, M. Am. Soc. C. E., discussed mathematically the nature and amount of the increase of resistance on curves due to the increase in number of cars upon a train, and deduced a general formula for the total tractive force necessary to be applied at the head of a train of cars moving at a uniform velocity on a given curve, and presented tables giving the coefficient for solving such formula in terms of the number of cars and the degree of curve. He also presented tables of resistances for given trains upon certain grades, and a summary of equivalent grades per station per degree for a given train upon various curves and grades. Also a table of resistances for a consolidation engine of 60 tons hauling its maximum train on a 20 degree curve. He also expressed the opinion that widening the gauge on curves was not an advantage as far as a four-wheeled truck is concerned.

Mr. Lewis Kingman, C. E., stated that on the Atlantic and Pacific Railroad the compensation $\frac{5}{100}$ per degree was adopted for all curves; that 10-degree curves were the maximum, and that all curves were cased off at both ends by compounding gradually from the tangent to the full degree of curvature. This practice he considered of great value and of very slight additional expense. From careful observation upon the action of locomotives pulling trains upon curves, it is his opinion that the compensation should vary with the grade.

Mr. A. M. Wellington, M. Am. Soc. C. E., said that the rule adopted by him was for the high grades, the same as that of Mr. Morley, viz: $\frac{4}{10}$ per degree of curvature, but that Mr. Morley increased this to $\frac{6}{10}$ per degree on lower grades. The theory upon which this was done was formerly advocated by Mr. Wellington, but he now believes from further investigation, both experimental and theoretical, that no sensible difference exists due to the longer trains which could be run upon the lower grades. No absolutely fixed rate of compensation ought to be made. Circumstances of location may make it inexpedient to adopt a rate of compensation which otherwise might be desirable. The rate of compensation of $\frac{6}{10}$ per degree is higher than is ever necessary unless in certain cases at stations or where it may become a question whether to admit certain sharp curves at all. It is extremely probable that curve resistance is materially greater at very slow speeds. It is unfortunate that further and more complete experiments cannot be secured than have already been made.

Mr. M. N. Forney, M. Am. Soc. C. E., referred to the fact that the conditions of rolling stock and of track were very important elements in any experiments that could be made upon this subject. Different car-builders made cars and trucks and wheels according to their individual notions. Engineers made the section of rail according to their individual notions. The actual condition of the rolling stock of the country was such that it was doubtful whether any experiments could be relied upon as determining results which could be applied generally.

Mr. Wellington expressed the opinion that while it was perfectly true that the condition of rolling stock was as stated by Mr. Forney, yet that experiments properly made could be relied upon to give fair average data.

The subject was further discussed by members present.

ENGINEERS' CLUB OF PHILADELPHIA— REGULAR MEETING, APRIL 5TH.—Past President Henry G. Morris in the chair; 36 members and 2 visitors present.

Mr. Henry G. Morris exhibited, on behalf of Mr. Israel W. Morris, two ancient and curious works upon mining: "The Golden Treasury, or the Complete Miner; being Royal Institutions or Proposals for Articles to Establish and Confirm Laws, Liberties and Customs of Silver and Gold Mines," by Thomas Houghton, London, 1699; and "A Collection of Scarce and Valuable Treatises upon Metals, Mines and Minerals," by James Hodges, London, 1740. The latter contains, *inter alia*, "How to know the Condition of the Earth by Taste," "Of Juices, and first of Allum." "The Opinion that Quicksilver and Sulphur are the Matter whereof Metals are made, is defined." "How to know the ill Qualities that infect the Oar, and how to Purge them away." "Wherein is showed how true and perfect Gold may be made by Art, with loss to the Workman," etc.

Mr. Henry G. Morris gave a brief description of an atmospheric elevator, consisting of a closed cage or car working in an air-tight well, the air pressure, supplied by a "root" or other pressure blower, being admitted to the top or bottom of the cage in descending or ascending. The doors at the different stories opening inwards, the pressure of air keeps them closed until the interior of the car is brought opposite, when the pressure being relieved the door can be opened into the car. The car being counterbalanced, only a comparatively slight pressure of air, equal to a water column of 6 to 8 inches only, is required to move an average load on a car six feet square. The escape of air beneath the car being at all times readily controlled by the attendant, it is impossible for the car to descend at a dangerous speed, and other obvious features render this form of elevator comparatively safe.

Mr. Henry G. Morris also exhibited a sample of seamless copper tube which had been compressed endwise under a steam hammer, and showed peculiar foldings of the metal into overlapping equilateral triangles forming an interior hexagonal section. The absence of fractures showed great purity of material.

Mr. John T. Boyd described a new design for parlor cars for the Pennsylvania R. R.

The Secretary presented, for Mr. Edward Parrish, an illustrated description of Powers' Disinfecting Tank and Automatic Siphon.

The Secretary presented, for the Reference Book, a table which he had prepared of vulgar fractions of 1 in. reduced to *exact* decimals of 1 in.

Mr. Wm. L. Simpson exhibited a remarkably perfect casting of a toad, the pattern used being the toad himself.

ENGINEERING NOTES.

AN ARTESIAN WELL, AT BOURNE, LINCOLNSHIRE. By James Pilbrow, M. Inst., C. E. The subject of artesian wells is not without interest to the engineer, whose attention is chiefly directed to the supply of towns and other places with water. For this reason, the description of a small but productive artesian well, completed at Bourne, in Lincolnshire, in 1856, is presented. The well was intended to supply the town of Bourne with water, the undertaking being in the hands of a small joint-stock company. The town had been until then without any public supply, and almost without a private one. The wells were shallow, as in most of the towns in that part of the county; but many houses were wholly dependent upon carts, which fetched water from a considerable distance. These circumstances gave increased importance to the fact of such a supply being found under the site of the place.

The boring, 4 inches in diameter, passed through several oolitic strata, to a depth of 92 feet. Below the alluvial soil and gravel, a hard shelly limestone, 32 feet in thickness, was encountered. The bore hole here was made slightly conical to admit of the taper end of a cast-iron pipe being inserted and driven tightly, to exclude any surface water, and to prevent water from the bore escaping into the gravel, and thus lose its full power to rise above the surface. The boring was then continued, through various beds, till it reached a stratum, 6 feet thick, of compact and hard rock, in passing through which, at 92 feet below the surface, the tool fell suddenly about 2 feet, evidently into a chasm or hollow, striking upon the hard surface of the underlying rock. The water immediately rushed up with great force, and drove the men from their work; and it was not without difficulty that the joints for attaching the curved pipe and sluice-valve at the surface could be accomplished.

The site of the town of Bourne partakes of the ordinary character of the county, and is flat; the highest part, where the well is situated, being only about 6 feet above the general level. It had been the intention of the author, should the water rise with sufficient force, as he believed it would do, to supply the town direct from the boring, and in this way the work was carried out, the flow and pressure having proved even greater than was anticipated.

An air chamber was fixed at the well to regulate the pressure, and to equalize the supply of water to the town. The water rose at the Town Hall exactly 39 feet 9 inches above the ground. The yield at the bore and surface level, ascertained by filling a tank capable of containing 5,000 gallons, was at the rate of 567,000 gallons per day, and there was no diminution on letting the whole run continuously to waste. The yield was also tested by a "notch-board," which, by using the coefficient 0.563, and measuring at still water and not at the "crest," gave 575,201.8 gallons.

The author knows of no other boring of like dimensions, either in this country or on the Continent, which yields so large a quantity of water, or where, the boring being made on the general level of the surrounding district, the water from which flows to so great a height above the ground.

It is needless to say that the town of Bourne has since enjoyed an unlimited supply of pure

water without the assistance of engine, pumps, or reservoirs, and in far greater quantity than it requires. The town of Spalding, several miles distant, has subsequently been supplied from the same source, the water being conveyed by pipes laid under the turnpike road.

The water mains were laid under every street, with fire-cocks at intervals, and it was satisfactory to all, and surprising to some, to see the water thrown upon the roofs of houses by a hose and jet-pipe, as from a fire-engine, and that only by the natural pressure of the spring.

The water, by Professor Brand's test, gave 19.4 degrees of hardness, arising chiefly from the presence of bi-carbonate of lime; but by boiling it is rendered much softer.—*Papers of the Institution of Civil Engineers.*

IRON AND STEEL NOTES.

THE AMERICAN BESSEMER STEEL INDUSTRY.—The following table shows the comparative production of ingots and rails by Great Britain and the United States in the last four years:

	1880.	1881.	1882.	1883.
Ingots.	Gross Tons.	Gross Tons.	Gross Tons.	Gross Tons.
Great Britain	1,044,382	1,441,719	1,673,649	1,553,380
United States	1,074,262	1,374,247	1,514,687	1,447,345
Excess, U. S.	29,880	—	—	—
Excess, G. B.	—	67,472	158,962	76,035
Rails.				
United States	852,196	1,187,770	1,284,067	1,148,709
Great Britain	739,910	1,023,740	1,235,785	1,097,174
Excess, U. S.	112,286	164,030	48,282	51,535

It will be observed, says the *Bulletin* (New York), that since 1880 Great Britain has annually made more Bessemer ingots than the United States, and that in the last four years the United States has annually made more Bessemer Steel rails than Great Britain. The shrinkage in the production of Bessemer steel rails in the United States in 1883 as compared with 1882 was 151,601 net tons, or 135,358 gross tons, which is 3,253 tons less than the shrinkage of 138,611 gross tons in the British production in the same period.

THE NEW B.G. WIRE GAUGE.—The following is a copy of the new B.G. wire gauge, which has been drawn up by the South Staffordshire Mill and Forge Wages Board, and submitted to the Board of Trade for approval:—

No. Gauge.	Inch.	Inch.	Millimetres.	Lb.
3/0	$\frac{1}{2}$	0.500	12 700	20
2/0		0.4452	11.308	17.808
1/0		0.3964	10.068	15.856
1		0.3532	8.971	14.128
2		0.3147	7.993	12.588
3		0.2804	7.122	11 216
4	$\frac{1}{4}$	0.250	6.350	13
5		0.2225	5.651	8.90
6		0.1981	5.032	7.924
7		0.1764	4.480	7.056
8		0.1570	3.988	6.28
9		0 1398	3.551	5.592
10	$\frac{1}{8}$	0.1250	3.175	5
11		0.1113	2.827	4.452
12		0 0991	2.517	3.964
13		0.0882	2.240	3.528
14		0.0785	1 994	3.14
15		0.0699	1.775	2.796
16	$\frac{1}{16}$	0.0625	1 587	2 50
17		0 0556	1.412	2.224
18		0.0495	1.257	1.98
19		0.0440	1.118	1.76
20		0.0392	0.996	1.568
21		0.0349	0.886	1.396
22	$\frac{1}{32}$	0.03125	0.794	1.25
23		0.02782	0.707	1.1128
24		0 02476	0 629	0.9904
25		0 02204	0.560	0.8816
26		0 01961	0.498	0 7844
27		0.01745	0.4432	0.698
28	$\frac{1}{64}$	0.015625	0.3969	0 625
29		0.0139	0.3531	0.556
30		0.0123	0 3124	0.492
31		0.0110	0.2794	0.440
32		0 0098	0.2489	0.392
33		0.0087	0.2210	0.348
34		0.0077	0.1956	0.300
35		0.0069	0.1753	0.276
36		0.0061	0.1549	0.244
37		0.0054	0.1371	0.216
38		0.0048	0 1219	0.192
39		0.0043	0.1092	0.172
40		0.00386	0.0980	0.1544
41		0.00343	0 0871	0·1372
42		0.00306	0.0777	0.1224
43		0 00272	0.0691	0.1088
44		0 00242	0.0615	0 096
45		0.00215	0.0546	0.0868
46		0.00192	0.0488	0 076
47		0.00170	0.0432	0.0688
48		0.00152	0.0386	0.0608
49		0.00135	0.0343	0.054
50		0.00120	0.0305	0.048
51		0.00107	0.0272	0.0428
52		0.30095	0.0241	0.038

THE OXIDIZABILITY OF IRON.—M. Gruner has published in *La Metallurgie* the result of a year's researches on the oxidizability of iron and steel under the influence of moist air, fresh, sea, and acidulated water. The results obtained are very instructive. Iron is dissolved rapidly by sea water, cast iron losing about half

as much as steel, and spiegeleisen is the most powerfully acted on by sea water.

LONG STEEL RAILS.—The Osnabruck Steel Works have recently been manufacturing steel rails 88 feet 6 inches long, which have been laid down on railway bridges crossing the city of Hanover. It was found that the noise caused by passing trains was becoming such a nuisance that a remedy had become a necessity. The cause of it was the violent vibration at the rail joints, and the engineers hit upon the expedient of having rails made long enough to cover the whole length of the bridges. Since they were laid down, the nuisance caused by the rail joints has ceased. The use of rails of the length stated is, as far as we know, without a parallel in the history of railway construction, and reflects credit alike on the engineers who suggested it and the manufacturers who made them.

TRIALS OF ARMORPLATES.—A competitive trial of armorplates took place on the island of Amager, near Copenhagen. There were four plates in all, one of steel from the Creusot Company, in France; one plate by Marrel, Rive de Gier, in France; and two compound plates from Sheffield, one made by Cammell. & Co., and the other by Brown & Co. These plates were 9 inches in thickness, and were about 6 feet 6 inches long by 5 feet wide, and were attached by bolts to a wood backing 9 inches in thickness, supported by iron skin plates and frames in the rear. The guns used were the 6 inch and 10 inch rifled guns, with steel projectiles. The shot from the 6-inch gun appeared to have much the same effect upon each of the plates, the Sheffield plates showing the least penetration. The shot from the larger gun knocked the left half of the Creusot steel plate completely away, thus laying open the target without any protection whatever from any other projectile, that might be fired at it. The same projectile passed through the other plates, but it was broken up by the English plates, which were not otherwise seriously damaged. It appeared as if the energy from the 10-inch gun was too great for any plate having a thickness of only 9 inches, but it was evident that the Sheffield plates proved to be the best under the exceptional conditions of firing.

RAILWAY NOTES.

A WRITER in the *Economiste Francais* describing the present condition of communications by water and railway in France, states that there are now 4,575 miles of navigable rivers, and 2,900 miles of canals. In the year 1852 there were only 4,190 miles of river navigable, and 2,440 miles of canal, the increase in the length of river being 385 miles, and in that of canals 460 miles. But, as the writer in the *Economiste Francais* points out, this increase has been effected at an enormous expense, the total amount spent from 1852 up to the year 1878 being close upon £14,000,000, while since then, under the scheme of M. de Freycinet for the development of communication by land or water, a further sum of £11,000,000 has already been spent. Thus in round figures the cost since 1852 has been £25,000,000 sterling, and yet the quantity of goods carried by water has only increased 4,000,000 tons. While the increase in the goods traffic upon the railways has been 4 per cent., upon the rivers and canals it has not exceeded 1 per cent., and this the writer in the *Economiste Francais* attributes, not to the unfair competition of the railways, but to the fact that canals cost as much to make as railways, and that the time occupied in transit is so much longer by water than it is by rail.

ORDNANCE AND NAVAL.

THE Russian Minister of War has ordered a large number of Krupp guns, which were procured from Essen some years ago, to be considerably lengthened, and they are at present undergoing this process at the St. Petersburg Arsenal. The barrels of the guns are first bored out to a considerable depth, and steel tubes of the required length are then inserted. Guns converted on this principle are reported to have given satisfactory results.

A steel gunboat for the French Navy was recently launched at Rochefort: she was named "Le Gabes," and is of the following dimensions: Length between perpendiculars, 149ft. 9in.; extreme breadth of beam, 23ft. 7in.; depth of hold, 10ft. 6in.; draught of water, 8ft. 6in. Her displacement ready for sea is 455 tons, and she will be fitted with engines of 450 indicated horse-power. It is estimated that she will realize a speed of upwards of 10 knots an hour.

BOOK NOTICES.

PUBLICATIONS RECEIVED.

PROCEEDINGS of the American Academy of Arts and Sciences; papers XXI., XXII., XXIII.

Geological Survey of New Jersey. Annual Report of the State Geologist for 1883.

Notes on Cylinder Bridge Piers. By John Newman, As. M. I. C. E.

Papers of the Institution of Civil Engineers; the following have been received from Mr. Jas. Forrest, Sec'y:

Water-Raising Machines in Holland, by G. Cupari.

Mining and Treatment of Gold Ores in Japan, by Robt. James Freckeville, Assoc. M. I. C. E.

Examination and Testing of Portland Cement, by Henry Faija, Assoc. M. I. C. E.

Timaru Water Supply, by Arthur Dudley Dobson, M. I. C. E.

Electrical Conductors, by William Henry Preece, M. I. C. E.

ENERGY IN NATURE. By Wm. Lant Carpenter, B. A., B.Sc. London: Cassell & Co.

This is a popular treatise designed to afford useful hints to teachers of physical science. The topics treated are Matter and Motion, Heat, Chemical Attraction, Electricity and Chemical Action, Magnetism and Electricity, and Energy in Organic Nature.

The book is tastefully printed and illustrated, and may be read with profit by young learners.

HEAT. By P. G. Tait, M. A. London: Macmillan & Co.

The intention of the author in preparing this work was to satisfy the wants of "the students who, without any intention of entering on a scientific career, whether theoretical or experimental, are yet desirous of knowing accurately the more prominent facts and theories of modern science to such an extent as to give them an intelligent interest in physical phenomena."

The book will doubtless be widely read, but the learner will be frequently at a loss to comprehend the exact meaning of the author, and at those points where special pains have been taken to be precise and clear.

TABLES FOR COMPUTING METRIC AND NON-METRIC WEIGHTS AND MEASURES. By W. A. G. Emonts, C. E. Philadelphia: William Syckelmore.

Inquiries are often made for convenient tables for reduction of weights and measures from the yard to the meter and *vice versa*. This little pamphlet of Mr. Emonts' fulfills the demand completely. In six pages of tables are all the ratios between the two systems which can reasonably be asked for.

THE PRINCIPLES AND PRACTICE OF ELECTRIC LIGHTING. By Alan A. Campbell Swinton. New York: D. Van Nostrand.

The aim of this work, as explained by the author, has been to prepare a treatise which should be on the one hand sufficiently simple and devoid of technicalities, to be easily understood by unscientific readers, and on the other, sufficiently comprehensive and up to date to give reliable information on all the principal appliances and systems. The plan is well carried out. Starting with the theory of electric lighting and explaining the nature of the problem to be solved, the author next gives a clear though popular account of the systems of units and the appliances for their measurement.

Sources of Power, Generators, Lamps, Accumulators, Systems of Lighting, are then presented in order The work closing with applications and their cost.

To people desiring the clearest and briefest exposition of this very interesting subject we recommend this book.

THE ART OF SOAP MAKING; a Practical Handbook of the Manufacture of Hard and Soft Soaps, &c. Including many new Processes, and a Chapter on the Recovery of Glycerine from Waste Leys. By Alexander Watt. With numerous Illustrations. London: Crosby Lockwood & Co.

Our London contemporary *Engineering*, in an introduction to a long review of this book, says:

"In a short preface, the author of this book modestly disclaims all pretension to originality, assuming only the credit due to an industrious compiler from many sources, and to a painstaking recorder of some of the chief processes in a great and ancient industry, unrepresented previously in this country by any special volume devoted to it. We think the readers of Mr. Watt's work will be disposed to accord to him more credit than he appears to consider his due, and manufacturers will doubtless derive much useful information from its pages."

The manufacture of many varieties of soap is described with special reference to the chemical and mechanical details, and to the improvement of new over old methods. Nine different modes of recovering the glycerine are described.

Useful notes and tables are appended, together with an excellent index.

MISCELLANEOUS.

M. HEDDEBAULT has discovered a method of preparing soluble wool from tissues in which wool and cotton are combined. When subject to a current of superheated steam, under a pressure of five atmospheres, the *Scientific American* says, the wool melts and falls to the bottom of the pan, leaving the cotton, linen, and other vegetable fibres clean and in a condition suitable for paper making. The melted wool is afterwards evaporated to dryness, when it becomes completely soluble in water, and is called azotine. The increased value of the rags is sufficient to cover the whole cost of the operation, so that the azotine is produced without cost. It contains all its nitrogen in a soluble condition, and can, therefore, be compared to dry blood, which is worth 2.50 francs per kilogramme of nitrogen. M. Ladureau regards this discovery as one of great interest for agriculture and mechanical industry.

DURAND has explained the spontaneous ignition of coal as being due to the presence of pyrites, which on oxidization under suitable conditions inflames and then sets fire to the coal in which it is imbedded. According to Fayol's experiments, however, the real cause of this phenomenon is the oxidation of the coal itself and not of the pyrites. The absorption of oxygen by coal—carbon—takes place more or less readily according to the temperature and the coal being more or less finely divided. According to the "Journal" of the Society of Chemical Industry, lignite in the state of fine dust takes fire at 150 deg., gas carbon at 200 deg., coke at 250 deg., and anthracite at 300 deg. or above. On heating a mixture of finely-powdered coal and pyrites to 200 deg. for four days the coal took up 6 per cent. of oxygen, whilst the pyrites absorbed only 3.5 per cent. Hence coal absorbs oxygen much more energetically than pyrites does, which has also been confirmed by the following experiment: About 900 grams of powdered coal and 3,350 grams of powdered pyrites were placed in tin cans and put in a drying chamber. Up to 135 deg. both behaved similarly, but from there the temperature of the pyrites remained almost stationary, whilst that of the coal quickly rose, ignition taking place after a few hours. Two other samples of coal and pyrites were put in a chamber at 200 deg. The temperature of the coal quickly increased. After forty minutes it got up to 200 deg., and the coal took fire, whilst the pyrites had at the same time only been raised to 150 deg. The ignition of the coal was not at all hastened by an admixture of pyrites.